

Robert Chapman
and Alison Wylie

Evidential Reasoning in Archaeology



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example they set as philosophers of science actively engaged with science practice and policy.

In the course of writing this book we realized that we were not alone in our frustration with the positions and debates we characterize as dead ends in thinking about evidential reasoning, and that many of our intuitions about more promising alternatives had been articulated by intellectual forebears who, to varying degrees, have been read out of our respective disciplinary canons. On the archaeology side, we have been particularly influenced by David Clarke's advocacy in 1973 for building an 'internal' philosophy of archaeology, and inspired by the example he sets for integrating philosophical reflection into research practice. And on the philosophy side, we found in Stephen E. Toulmin's brief for attending to the *Uses of Argument* (1958) an articulation of a philosophical stance that anticipates by several decades the pragmatic turn we hope is successfully embodied in our case-grounded analyses of evidential reasoning in archaeology.

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Introduction: The Paradox of Material Evidence

The allure of archaeology is, for many, the thrill of an encounter with tangible traces of a human past irrevocably beyond reach, but somehow made present with an intensely compelling immediacy. A compendium of *Wonders of the Past* dating to the 1920s, brimming with enthusiasm for discoveries of the previous century, trades on just this sense that, whether these traces are grand monuments and ancient art or the surviving fragments of daily craft production, they put us in uniquely direct contact with the past. In the absence of the actual ruins and artefacts, lavish illustrations promise to convey what archaeology has to teach better than descriptions could do:

They are visible evidence of the cultures and civilisations which have passed away, of the marvellous achievements of human genius and art which have been ruthlessly mutilated or destroyed. They enable us to see once more a fragment of what civilised man has achieved in earlier ages and in other lands, to lift the veil that hangs over his life and mental powers in the distant past. (Sayce 1923/33: 9)

Narratives of discovery convey the power, and sometimes the shock, of material confrontation with the past in particularly vivid terms. Perhaps the most famous of these is the romantic story of Schliemann's great discovery at Troy in 1873: working alone with his young Greek wife, Sofia, to forestall the risk of thievery, 'the pair brushed the dirt from gold vessels and diadems', exulting in the momentous discovery of the 'great Trojan treasure', Priam's hoard (Klejn 1999: 110, 115).¹ Evidently the hoard itself was assembled from a number of different sources, and the story of its discovery a blatant fabrication – a 'florid archaeological fantasy' (Gere 2009: 23–4) – but at the time it immediately captured public imagination as tangible evidence that Schliemann had located the famous city of Homeric legend.

Less romantic but, if anything, more consequential are the accounts recorded throughout the nineteenth century of a growing number of

human remains and artefacts found in association with the bones of extinct Pleistocene mammals in stratified deposits, discoveries that were pivotal in challenging the biblical time frame that had sharply delimited the horizons of human history. In one of the most famous of these, Paviland Cave, William Buckland discovered a seemingly undisturbed, ochre-covered burial with a rich array of worked ivory that came to be known as the Red Lady of Paviland. A figure of legend, she inspired an astonishingly diverse array of mythologizing narratives about the past that she represented. She was at one point an excise man murdered by coastal smugglers and then, when the associated burial goods convinced Buckland that she was a woman, a witch, a high-born Welsh ancestor, or a Roman army camp-follower of questionable character (Sommer 2007: 62–70; Grayson 1983: 64–9).

While these examples of dramatic discovery vividly illustrate how material finds can mobilize the projection of personal ambitions and collective imaginings, they also bring into sharp focus the capacity of surviving traces to bear witness to pasts that are otherwise unimagined or unimaginable. Whether it is the grandeur of a lost civilization, the crumbled legacy of an Ozymandias encountered in the desert sands, the skeletal remains of unexpectedly ancient forebears, or worn tools and shattered bones that testify to the hardship of marginal lives in more recent contexts (Blakey 2008), time and again material evidence has proved to be a powerful corrective to the myopia of presentism and the elitism of much text-based history. The history of archaeology is replete with examples of reasoning from material evidence that challenges entrenched dogma, countering inadvertent ignorance and opening up vistas on this ‘foreign country’ that is the past that no one had even thought to explore (Lowenthal 1985). This paradoxical robustness of material evidence as an epistemic resource has attracted the attention of historians and is reflected in the epistemic optimism expressed by the advocates of ‘object studies’, a rapidly growing, vigorously interdisciplinary field (see Wylie and Chapman 2015: 1–5). For example, the historian Lorraine Daston reflects on the ‘brute intransigence of matter’ (2008: 11) as giving reason for a certain optimism about what historians can learn from surviving traces (2008: 15–16). She notes a long tradition of thinking about things – in European legal codes, Baconian science,

Christian theology – in which, despite its often enigmatic status as evidence, ‘the talking thing’ is valorized as that which ‘spoke the truth, the truest, most indubitable truth conceivable ... [because] it had been uttered by the things themselves, without the distorting filter of human interpretation’ (2008: 13). Neil MacGregor describes the motivation for *A History of the World in 100 Objects* as an ambition to ‘tell a history of the world’ that is more comprehensive, ‘truer ... more equitable than one based solely on texts’ (2010: xv, xxv, xix). Historical archaeologists have been especially forthright in claiming this epistemic high ground from the inception of their discipline-bridging field. For example, Henry Glassie and Robert Ascher insisted that material evidence is not just a supplement to text-based histories but often the only resource we have for exposing and correcting ‘superficial and elitist ... myth[s] for the contemporary power structure’ (Glassie 1977: 29), the systematic distortions that arise from ignoring ‘the inarticulate’ (Ascher 1974: 11). On this account, archaeology as a discipline is a family of theory-rich empirical research practices designed to systematically exploit the belief-challenging, horizon-expanding capacity of material traces as evidence of the human, cultural past.

In practice, of course, realizing this promise is never as simple as suggested by Schliemann-style ‘confrontation-with-the-past’ narratives of discovery. The data rarely ever speak with a single voice, self-warranting and prophetic; what they say is never unmediated by interpretation.² The complexity of the interpretive process is radically misrepresented by hackneyed analogies with solving a jigsaw puzzle. Despite the proliferation of floridly imagined pasts, the ochre-stained skeleton that Buckland discovered did play a central role in nineteenth-century debates that culminated, in the 1860s, in a provisional consensus that human antiquity was considerably deeper than proposed on the basis of the framework of biblical chronology within which material evidence of human ancestors had been understood. But the trajectory of this debate was never smooth, and in the 1870s this first ‘Antiquity of Man’ debate gave way to a second that took at least five decades to resolve. The dramatic reframing of the time depth of human history that took shape over a century required not just the discovery of a growing number of puzzling

remains like those of the Red Lady but also the development of background knowledge in a number of key areas: in geological understanding of the stratified cave and gravel deposits in which these remains were found; in the use of a growing roster of analytic techniques to assess the age and composition of the skeletal remains themselves; and through the reinterpretation of puzzling stone ‘eoliths’ as human tools, a process that depended on recruiting the resources of ethnographic analogy and the results of early taphonomic experiments to distinguish natural fracturing from the products of deliberate knapping. The fortunes of the shifting array of hypotheses that were explored, refined, rejected and reconfigured in the context of these debates – about humans as ancient but post-diluvial, then post-glacial, then dubiously in the Tertiary and finally firmly rooted in the Quaternary – also depended on successive refinements of Darwinian evolutionary theory and its contentious extension to biological humans and their social and cultural worlds. All of this was, in addition, heavily inflected by profound shifts in the *fin de siècle* intellectual culture, the contradictory romantic and secular impulses of modernism described by Gere (2009); the impact of industrial capitalism, instrumental to the discovery of many of the central finds; and the massive expansion and reconfiguration of the antiquarian societies, and the museum and emerging university-based professions, that made possible the formation of the disciplines of geology, archaeology and palaeontology.³ With this array of technical, conceptual, theoretical and institutional scaffolding in play, by 1913 it was established that the ‘Red Lady of Paviland’ was male and, in the 1960s, that his remains date to the Upper Palaeolithic, approximately 26,000 BP (Grayson 1983: 67) – no whiff of a Roman prostitute or ancient Celtic witch in sight!

In short, the recognition of an antediluvian human (pre)-history was a hard-won accomplishment, realized against stiff resistance on a number of fronts, contingent in its course and outcome, and dependent on a broad array of background knowledge, including both technical and conceptual resources drawn from collateral fields, all of which were rapidly evolving and often contentious in their own terms. As such, the Red Lady of Paviland and the fortunes of Schliemann’s claims to have discovered Priam’s Troy⁴ make clear how complicated

it is to read surviving traces as evidence and yet, at the same time, how stubbornly recalcitrant these data can be, no matter how entrenched their assumed meaning comes to be. Even when surviving traces are without question game-changing archaeological discoveries, understanding their import as evidence is a painstakingly hard process of learning to see and, crucially, learning to ‘see as’ (Hanson 1958). This is not unique to archaeology; Hasok Chang describes just such a process as it unfolded in the centuries-long struggle by physical chemists to establish a reliable system for measuring temperature (2004). As much as we take thermometers for granted now, it was by no means clear in the seventeenth and eighteenth centuries, even in much of the nineteenth century, what should count as the ‘fixed points’ to which temperature scales could be anchored. There were no absolute foundations to which experimenters could appeal; they had to be constructed through a process of successive approximation – what we will refer to as a matter of scaffolding and bootstrapping (see [Chapter 1](#)) – by which chemists relied on assumptions and methods they knew to be faulty but that made it possible to refine their understanding of the phenomenon of temperature to the point where they could eliminate some initial hypotheses and articulate new, more sharply specified questions, questions that would require the construction of new scaffolding. Chang describes this process of ‘epistemic iteration’ as both conservative, embodying a ‘principle of respect’ for previously accepted standards, and driven by an ‘imperative’ to progressively refine and reach beyond these provisional foundations (2004: 43–4). It is, he says, a ‘valid and effective method of building scientific knowledge in the absence of infallible foundations’ (2004: 231).

The cases we discuss in the chapters that follow bring into sharp focus the scaffolding of various kinds – laden theory, background knowledge (tacit and explicit), technical skill, social networks, institutional infrastructure and vigilant reflexive critique – required to make archaeological observation possible and to put the resulting data to work as evidence. Neither these data nor the evidential claims based on them constitute a self-warranting empirical foundation, and yet they can powerfully challenge and constrain the reconstructive and explanatory claims we project onto the cultural past. This is the

paradox of material evidence: that ‘traces don’t speak’. Material evidence is inescapably an interpretive construct; what it ‘says’ is contingent on the provisional scaffolding we bring to bear. And yet it has a striking capacity to function as a ‘network of resistances to theoretical appropriation’ that routinely destabilizes settled assumptions, redirects inquiry and expands interpretive horizons in directions no one had anticipated – a capacity acknowledged by even the most vigorously anti-foundationalist critics within archaeology (Shanks and Tilley 1989: 44).

This paradox of material evidence lies at the heart of archaeology. It has been the catalyst for ongoing creative innovation, methodological and conceptual, that has generated some strikingly transformative insights into the cultural past, but at the same time it is a perennial source of epistemic pessimism. The question we take up in this short book is, then, how are the successes realized? And how are specific risks of error, distortion, elision and the arbitrary projection of expectations most effectively countered? We proceed on the conviction that considerable wisdom is embodied in the creativity and skilled practice of archaeologists that is only made explicit when trouble arises, typically in the context of close-to-the-ground debate about specific cases. We also take inspiration from a caution issued by David Clarke in the mid-1970s: this wisdom is not likely to be well captured by idealized accounts of ‘scientific’ (or historical) practice constructed by philosophers in response to their own internal debates and modelled on what is often a simplistic understanding of fields that lie at considerable distance from archaeology, usually the physical sciences (1973).⁵ Our aim is to capture the strategic wisdom-in-practice that lies between tactical norms and abstract theoretical ideals. If we succeed, much of what we present should seem obvious. But we hope that, in giving this wisdom explicit formulation, we will make it more widely accessible and contribute to the ongoing process of learning from collective experience that is characteristic of archaeology at its best.

In [Chapter 1](#) we revisit the debate about epistemic ideals that has been a recurrent source of contention within archaeology. It is here that the paradox of evidence is framed in its starkest, most divisive terms as a dilemma animated by anxieties about the security of

archaeological evidence that are generalized into all-or-nothing epistemic stances: if archaeologists set their sights on establishing claims that are empirically irreproachable they may foreclose (some) risks of error but at the expense of abandoning the very questions that make archaeology worth doing, and if they do not self-limit in this way they may have nothing to offer but speculation. These stances are evident in practice in the division between archaeologists who bear down closely on the data and those who range beyond the data, making bold conjectures and interpretive leaps. The most recent round of ‘theory wars’ – the long-running contretemps between the processual New Archaeology and the diverse array of post-processual initiatives mobilized by its critique⁶ – has dissipated in the last two decades, but the underlying issues that they raise for archaeology have not been resolved (Johnson 2010: 220–3). We argue that the intransigence of this debate – the vacillation between epistemic despair and aggressive optimism – is largely a function of the way it has been framed: in terms of abstract ideals of scientific certainty and objectivity that are unattainable and that provide little guidance for making well reasoned judgements of evidence-based claims in a real world where these inevitably trade in degrees of plausibility and credibility rather than certainties. There are a number of less simplistic models of evidential reasoning on offer – in informal logic, practice-grounded philosophy of science and a growing body of philosophical work on the historical sciences – that provide a framework for characterizing the dynamic process by which archaeologists develop the various kinds of scaffolding they need to interpret data as evidence, exploit the capacity of multiple methods and lines of evidence to constrain one another and leverage what they learn to continuously rebuild and extend these provisional foundations. This process enables archaeologists to meet constructively the challenge issued by Binford, the dominant force behind the New Archaeology: ‘how to keep our feet on the “empirical” ground and our heads in the “theoretical” sky’ (1981: 21).

We then consider how these strategies play out in practice: first in the context of fieldwork ([Chapter 2](#)) and then in connection with the various means by which archaeologists induce old evidence to tell new stories ([Chapter 3](#)). It is in the recovery and recording of their

primary data that archaeologists put in place scaffolding that is fundamental to the enterprise as a whole: the skills of identification and observation, the development of new ‘ways of seeing’ data (Bradley 1997), the conventions of recording and analysis that ‘capture’ the material traces that will constitute the empirical basis for evidential claims (Chippindale 2002). As critics of empiricist and positivist research programmes in archaeology have repeatedly pointed out, there is no theory-free, self-warranting foundation of evidence to be found in the stuff of the archaeological record; the empirical foundations of interpretation are themselves an interpretive construct, all the way down. The paradox of evidence first intrudes, then, in the context of fieldwork. It is here that we consider how, as problem-specific, selective and regimented as the conventions of primary data recovery and recording must be, the data recovered can none the less constrain interpretation and sometimes sustain the surprise of unexpected discovery. It is in the creative uses archaeologists make of legacy data that this capacity of things to resist appropriation is most clearly on display. In [Chapter 3](#) we focus on empirical and conceptual factors that make up the scaffolding arguments by which old data are put to new uses. We consider examples drawn from recent reappraisals of evidence generated by the long-running tradition of ‘Moundbuilder’ research in North America and develop an extended case study of the successive rounds of critique and reuse of data recovered by late nineteenth-century excavations of the Iron Age village of Glastonbury. Taken together, these illustrate how, even when the primary data are known to be incomplete and in various senses untrustworthy, they can none the less function as productive catalysts for new lines of research.

We turn, in [Chapter 4](#), to consider the role played by external resources in expanding the range of data available to archaeologists and the reliability of their interpretation as evidence. The central insight here is that there are no silver bullets. No matter how scientifically or technically sophisticated it may be, the scaffolding drawn from neighbouring fields rarely establishes archaeologically relevant evidential claims without extensive reinforcement and calibration that depends on a great many internal (archaeological) and external resources. This is especially clear in the case of the multiple

radiocarbon revolutions that have transformed archaeological dating, the first of three cases we consider in [Chapter 4](#). We juxtapose with this famous success story two contrasting examples of the use of different types of isotope analysis that illustrate what can go wrong and what is required for success in bringing external resources to bear on archaeological problems: the contentious history of debate in the UK about lead isotope analyses of Bronze Age copper artefacts (metals moving); and the dietary reconstructions that have been developed by the ‘Diaspora Project’ to address questions about migration within the Roman Empire (people moving). We describe the process of recruiting and fine-tuning external resources for archaeological application as a process of triangulation in which, in the absence of self-warranting foundations, archaeologists play one line of evidence against another, using each to constrain and extend the other. Pulling together the threads of these arguments and examples, we give an account of the epistemic rationale that underpins archaeological practices of ‘robustness’ reasoning that depend on mobilizing multiple lines of evidence.

Just as there are no technical fixes that can displace archaeological judgement, so too there is no methodological guarantee that the process of patiently building, testing, cross-checking and calibrating a diverse suite of evidence will be self-correcting where framework assumptions are concerned. And yet, the history of archaeology is replete with examples of challenges to foundational beliefs that were so deeply entrenched, so much taken for granted, that their role in shaping the trajectory of archaeological inquiry was unrecognized until they were called to account. Indigenous, feminist, race and class-based critiques are key examples of such challenges and in each case the resources of a self-consciously situated, often explicitly political standpoint play a central role in mobilizing epistemic critique. We argue that contextual values and interests cannot be eliminated but, contrary to ideals of objectivity that trade in the fiction of a ‘view from nowhere’ (Nagel 1986), this should not be considered a counsel of despair. The final question we address in the Conclusions chapter is how a commitment to critical reflexivity can be made concrete: how research communities can put the resources of diversely situated epistemic agents to work to ensure the possibility of such

transformative criticism.

What we offer, then, is a close analysis of the various types of scaffolding that enable archaeological research and an argument for seeing the ongoing revision of this scaffolding not as evidence of a failure to locate empirical bedrock, but as a mark of success: an indication that archaeologists are engaged in a dynamic process of continuously building, extending and refining provisional foundations. In the course of this analysis we identify a number of epistemic norms that we believe will serve archaeologists better than the all-or-nothing ideals of truth and objectivity that tend to dominate programmatic debate about the scientific status of evidential reasoning and of archaeology itself. These include virtues of epistemic humility captured by the principle of respect for the usefulness and also the provisionality of the scaffolding built by those who have come before that Chang (2004) describes. In archaeological contexts, Joan Gero articulates a related virtue in terms of a commitment to ‘honor ambiguity’ (2007): a brief for keeping in view the contingent, context- and purpose-specific nature of the norms of practice and background assumptions on which archaeologists must rely. A respect for ambiguity and for unexplained observations is the final nail in the coffin of the jigsaw-puzzle metaphor that is still often invoked in accounts of archaeological interpretation. Another virtue, ambivalently embraced but ubiquitous, is an expansive and sometimes wildly eclectic methodological and theoretical opportunism⁷ that requires the cultivation of a working knowledge of resources – conceptual, empirical, technical – developed in a rich array of external fields that may be relevant to archaeological problems. Although the quest for a defining, uniquely archaeological methodology is a recurrent theme in internal debate, we argue that the distinctive successes of archaeology as a discipline could only have been realized by constructing the field as a ‘trading zone’, in a sense like that suggested by Peter Galison (2010),⁸ its boundaries permeable and its practitioners conversant enough in the languages and practices of dozens of other fields to bring radically diverse resources to bear on archaeological problems.

Finally, we offer a reformulation of ideals of objectivity; we argue that the virtues of practice we identify in earlier chapters are best

captured by a pragmatic and procedural conception of objectivity, along lines suggested by Helen Longino's account of norms of critical engagement that are instantiated in and required of well functioning scientific communities (2002). On this account, the goal of inquiry is not to produce knowledge claims that are true in all contexts of practice and transcendent of local interests, but rather to warrant knowledge claims as credible given available resources, and reliable for specific purposes. Objectivity is, then, characterized in terms of norms of practice that, together, secure the trustworthiness of specific knowledge claims as fit for purpose. In addition to the epistemic virtues we distil from analysis of archaeological best practice, these norms include requirements of rigour, integrity and transparency in the collective appraisal of knowledge claims that make them accountable to their contexts of production. The various cases we consider illustrate a range of degrees of success and failure in realizing such norms; some exemplify the central virtues of critical engagement continuously and on many fronts, while others are a sobering reminder of the fallibility of the archaeological enterprise. But even the most problematic examples are double-edged; however much they illustrate pitfalls, they also throw into relief what could have been done, and what can be done in the future, to counteract the ever-present risks of local and systematic error. Together with more hopeful cases and those of mixed success, they illustrate why polarized debate about the epistemic status of archaeology is fundamentally misconceived. Material traces and the methods archaeologists use to constitute them as evidence are just too diverse and open-ended to sustain categorically pessimistic or optimistic conclusions. They require case-by-case assessment, a matter of concretely grounded reflexive analysis that is itself a crucial dimension of the bootstrapping practices we find characteristic of the best of archaeology.

Notes

In excavating this wall further and directly by the side of the palace of King Priam, I came upon a large copper article of the most remarkable form, which attracted my attention all the more as I thought I saw gold behind it ... In order to withdraw the treasure from the greed of my workmen, and to save it for archaeology ... I immediately had 'paidos' [lunch break]) called ... While the men were eating and resting, I cut out the Treasure with a large knife ... It would, however, have been impossible for me to have removed the Treasure without the help of my dear wife, who stood by me ready to pack the things which I cut out in her shawl and to carry them away.

2

Gere (2009) explores the cultural context and history of Schliemann's and Evans's archaeological exploits, characterizing them as self-made 'prophets of modernism'. For a sustained and uncompromising critique of the presumption that archaeological data are somehow a 'yardstick' against which text-based claims about antiquity can be decisively tested, see Ullmann-Margalit (2006: 45). She develops this point through close analysis of the interplay between textual and archaeological interpretation of the Dead Sea Scrolls, at several junctures emphasizing the point that 'finds are not raw material: they are materials under interpretive description' (p. 46).

3

Toulmin and Goodfield (1965) provide a classic account of the complex, extended, multidisciplinary process by which this was accomplished, and Grayson (1983) details the contingent process by which French and, later, British researchers came to recognize geological time depth and stratigraphic sequencing in the contexts where Pleistocene fauna were found in association with human remains and tools. Sommer uses a sharply focused object biography of the Red Lady of Paviland as the lens through which to trace the interplay between diverse bodies of background knowledge, expectation and rapidly evolving research practices that configured this skeleton as an object of scientific investigation (2007: 9).

4

Schliemann did not systematically record or analyse the stratigraphy of the site he claimed was Priam's Troy. Subsequent reanalysis suggests that the treasure that made Schliemann famous dates from several centuries earlier than the period when Priam is assumed to have been king of Troy.

5

See, for example, a strikingly parallel critique published in the same year by Mario Bunge, a philosopher of science. He particularly objects to philosophical accounts of science that rely on what he refers to as preface and textbook analysis: simplified accounts of scientific inquiry that reinforce a philosophical fixation on idealizations that are often radically ‘out of touch with [real] science’ and which misdirect philosophical attention to pseudo-problems (1973: 2, 4–6).

6

For an account of the New Archaeology and the debate with its post-processual critics, see Chapter 1.

7

Currie describes the historical sciences – chiefly palaeontology and geology – as characterized by ‘methodological omnivory’ (Currie 2014a, 2014b), an account that captures exactly the ‘trading zone’ practices we find ubiquitous in archaeology.

8

Galison describes boundary-crossing translational practices that have proved transformative in collaborations between theoretical physicists and engineers, computational modellers and experimentalists (2010). His account is expanded upon by sociologists of science Collins, Evans and Gorman (2007) and contributors to *Trading Zones and Interactional Expertise* (Gorman 2010). We find various of the trading zone practices they describe at work in archaeology, not only as a strategy for addressing problem-specific interfield engagements but as a characteristic of the field as a whole.

Archaeological Evidence in Question: Working between the Horns of a Dilemma

‘An element of conjecture which cannot be tested’

In the mid-1950s a field archaeologist who worked extensively with Christopher Hawkes presented a short paper on ‘[The Limitations of Inference in Archaeology](#)’ to the Prehistoric Society at the Institute of Archaeology in London. She was identified as Miss M. A. Smith,¹ and she clearly drew on philosophical as well as archaeological training. In this article, which appeared in the *British Archaeological Newsletter* in 1955, Smith argued that although the ‘assimilation of archaeology to scientific practice’ had greatly expanded the range and improved the quality of the data recovered by archaeologists, providing them with ‘precise determinations’ of their finds, these accomplishments had come at a cost. The growing emphasis on technical competence did tend, she observed, to ‘have a rather hypnotic effect on the mind’; it carried the risk that archaeologists would lose their sense of purpose as practitioners of a human, historical science (1955: 1). But rather than defend an ambitiously humanistic archaeology – one dedicated to grasping the distinctively human, social dimensions of the cultural past – Smith went on to argue that archaeology is inherently limited in what it can hope to understand of the vast reaches of human history that are, as she put it, ‘undocumented’ (p. 2).

The reason for Smith’s pessimism was the appraisal that there is ‘no logical relation’ between the social, cultural past and its surviving record. The only inferences from archaeological data that she recognized as ‘legitimate’ are those for which ‘all the evidence can be empirically verified’ and ‘nothing has been added’ (p. 2); the inferential standard she invokes here is what philosophers would describe as ‘truth-preserving deductive entailment’.² As she points out, when this level of certainty is realized, the conclusions drawn are

really nothing more than ‘a *paraphrase* of empirical observations’ as these are presented in the premises of the argument (p. 2). If archaeologists adhered to such a standard, she asks, how could they ever grasp the form of life, the animating aesthetic sensibilities and social relations of, say, Trobriand Island society as described in rich ethnographic detail by Bronisław Malinowski? No matter how ‘perfectly conducted’, the archaeological excavation of a Trobriand village could never carry archaeologists any distance towards an understanding of the complexity of Trobriand society, nor even give them a very robust understanding of the agricultural practices, local economy or demography of these communities; ‘it would obviously be impossible’, she concludes, ‘to understand the relics of the Trobrianders from the evidence of the material remains alone’ (p. 3). She then draws from this example a quite general lesson that she refers to as the Diogenes problem. Quoting Mortimer Wheeler, she observes that ‘the archaeologist may find the tub ... but altogether miss Diogenes’ (p. 2; Wheeler 1950: 130) and, moreover, may have no way of ever knowing what they have missed.³

Smith’s argument for this conclusion depends on two sets of claims. While she emphasizes the epistemic and methodological challenges that archaeologists face working with an incomplete material record of the cultural past, she also invokes a more fundamental, ontological⁴ problem that arises from the nature of the subject of inquiry. She argues that, by their very nature, the cultural subjects of interest to archaeologists must elude understanding; the relationship that holds between surviving material traces and the social contexts and actions that give them cultural significance is radically unstable. Although Smith does not develop this point in any detail, she invokes a ‘normative’ or ‘ideational’ conception of culture: the view, most systematically developed in an archaeological context by Walter Taylor, that culture is a ‘mental phenomenon’; it consists of community norms and conventions that inform behaviour and are manifest in material culture, these being ‘objectifications’ of culture proper (1948: 49). This resonates with an influential Wittgensteinian argument against the very ‘idea of a social science’ put forward in philosophical terms by Peter Winch, an Oxford-trained contemporary of Smith’s (Winch 1958). In his account, social actions, as social, are

distinctively ‘rule following’, not law-governed; they are intentional, not mere stimulus-responsive behaviours. In this respect, social, cultural subjects are not just more complex or more chaotic than natural phenomena; they are categorically different. To focus on behavioural regularities an effort to emulate the natural sciences is to ignore the beliefs and intentions, the shared understandings and collective norms that give actions meaning, and it is only by grasping the meanings of actions, Winch argued, that they can be understood. The goals of law-governed explanation and prediction (or retrodiction) typical of the natural sciences are inadequate to the study of social phenomena; capturing regularities in behaviour provides no insight into the reasons and intentions of actions that constitute them as social (see his [Chapter 3](#), ‘The Social Studies as Science’). Smith’s worry is that, even with the most complete material record imaginable (a Pompeii or an Ozette), if you cannot invoke uniformitarian law-like premises about the connections between traces and their antecedents, it is impossible to secure interpretive conclusions with the degree of certainty she requires. Between ‘the human activities we should like to know about’ and the ‘visible results which survive from them’ there is, she argues, ‘logically no necessary link’; therefore it is ‘a hopeless task’ to attempt to move from one to the other ‘by argument’ (Smith 1955: 4).

The upshot is that, according to Smith’s account, any inference that goes beyond empirical description of the surviving material traces must be understood to ‘contain an element of *conjecture*, which cannot be tested’ (p. 3); it is inescapably speculative. Better for archaeologists to trim their sails, she urges, than indulge in the vain hope that ratcheting up scientific rigour in the recovery and analysis of material traces will fill this inferential gap. A lesson that archaeologists should learn from the sciences, Smith counsels, is to respect the ‘insuperable limits to what can legitimately be inferred from archaeological material’ (pp. 4–5).

In fact, Smith’s pessimism is even more undermining than she acknowledges, inasmuch as observational claims about material traces – certainly any claims that involve identifying them as archaeological – are already, themselves, highly selective and richly interpretive. The ‘element of conjecture’ that worries her afflicts the empirical premises

of the inferences she considers inherently insecure as much as they do the more ambitious conclusions about the cultural past that she declares out of reach.

Crisis debates⁵

As extreme as Smith's line of argument may seem, it is by no means an isolated example of epistemic anxiety about the status of archaeological evidence and the grounds it provides for understanding the cultural past. Indeed, these worries are not unique to archaeology,⁶ but archaeologists have been especially reflective about the fact that they have no alternative but to depend on evidence that is, as one commentator put it, 'remote from and uncertainly coupled to the systems [they] seek to study' (Chippindale 2002: 606). The 'proxy status' of archaeological evidence entails a reliance on 'auxiliary' hypotheses – background assumptions, 'gap-crossers', 'middle-range theory'⁷ – that establish the connections between surviving traces and the past events, conditions and actions of which they are presumed to be evidence. As for Smith, these more recent worries are both epistemic and ontological. Material traces are subject to the vagaries of destruction, dispersal and distortion; confounding causal factors are always a possibility to be reckoned with; and, in a great many cases, even if these can be ruled out, the same configuration of traces can plausibly be explained in terms of different originating and mediating conditions. This problem of equifinality is especially acute when the subject of inquiry is a symbolically rich material culture that may embody (by intention or accretion) an array of different meanings; a problem of (epistemic) underdetermination⁸ is thus amplified by the indeterminacy of the subject of inquiry itself.

To make matters worse, these worries are sometimes amplified and generalized by appeal to the 'theory-laden' nature of archaeological evidence.⁹ If material traces can only be recognized as archaeological and put to work as evidence when interpreted in light of some background theory, so the argument goes, then the conclusions drawn are always vulnerable to what Bell describes as xeroxing (2015: 42, 45): the imposition of interpretive conventions that moulds material traces to contemporary 'pre-understandings' (Hodder 1999: 49–52).

Furthermore, this suggests that archaeological evidence is inherently unstable; depending on the background theory archaeologists bring to bear, they may interpret the significance of their data as evidence very differently; indeed, their assumptions may predetermine what they will find. At best, then, what we count as archaeological evidence is subject to two orders of sampling bias: first, the biases introduced by the vagaries of survival of material traces and, second, the limitations imposed by the resources archaeologists draw on to recognize, recover and interpret these traces as evidence. At worst, the most deeply pessimistic commentators worry that evidential reasoning in archaeology is nothing but the projection of contemporary expectations onto the past. The shared insight here is that archaeological evidence is not an autonomous, self-warranting empirical 'foundation', different in kind from the theoretical claims it supports or is used to test; it is, itself, an interpretive construct. As such, it cannot secure claims about past events with the kind of certainty required by Smith. In its most uncompromising form, this 'constructionist' line of argument¹⁰ leads to the conclusion that appeals to archaeological evidence never escape the threat of vicious circularity;¹¹ if empirical data are inevitably interpreted in terms of 'pre-understandings', the worry is that the evidential claims archaeologists base on these data cannot but conform to their expectations.

Anxieties of this sort have provoked a series of crisis debates within archaeology that go back at least a century and follow a predictable pattern. In the early twentieth century the advocates of a First World War-era 'New Archaeology' insisted, in opposition to an antiquarian preoccupation with 'the mere finding of things', that archaeologists must set their sights on genuinely anthropological questions about the past and embrace scientific methods in addressing them (Wissler 1917). 'The time is past when our major interest was in the specimen', declared Roland Dixon (1913: 564); to move beyond the 'woefully haphazard and uncoordinated' practices of their antiquarian forebears, he argued, archaeologists must focus their attention on gathering evidence relevant to building and testing 'working hypotheses' about 'the relations of things ... the whens and the whys and the hows' (p. 565). To this end, Dixon argued, it would be necessary to develop a

rich body of cultural theory, including both general theory about cultural systems and processes, and an understanding of specific cultural forms grounded in contemporary ethno-history; only given these resources would archaeologists be able to link surviving material traces to antecedent cultural forms and begin to make use of these data as evidence to refine and test explanatory hypotheses about the past. This call for 'saner and more truly scientific methods' (p. 563) was immediately met by criticism from those who objected that any attempt to theorize was premature, little more than a licence to speculate that could only undermine the integrity of the very evidence archaeologists were working so hard to recover; the priority for properly scientific archaeology must be to build a robust body of 'facts' and defer theorizing until the evidence was in (Laufer 1913).

Despite repeated declarations that the field had been professionalized – that by mid-century archaeology was 'completely divorced from the business of collecting curios and the stigma of antiquarianism had practically disappeared' (F. Johnson 1961: 2) – frustration that more ambitious historical, anthropological goals were still not being addressed effectively resurfaced in the late 1930s, the 1950s and again with the (new) New Archaeology of the 1970s. Clyde Kluckhohn objected that many archaeologists remained 'but slightly reformed antiquarians' (1940: 43); the recovery of 'facts' from (or about) the archaeological record was still a central preoccupation. Over the next thirty years a succession of internal critics challenged 'traditional' modes of practice that were chiefly concerned to bring systematic order to these facts, constructing cultural histories that were little more than a gloss on descriptive claims about the formal variability and spatial-temporal distribution of archaeological material.

At each juncture when the advocates of a more robustly anthropological archaeology have challenged the wisdom of deferring explanatory goals, the lines of dispute have been strikingly similar. On the one hand, there are those who insist that data collection must be the first, foundational stage of archaeological inquiry. So, for example, in a North American context Taylor argues that archaeological inquiry, *qua* archaeology, is 'no more than a method or set of specialized techniques for the gathering of cultural information', and

the archaeologist ‘nothing but a technician’ (1948: 41). What archaeologists do with these data once they are recovered will determine whether they contribute to historical or anthropological understanding but, as archaeologists, they should operate with just one objective: ‘to exploit fully and without abridgement the cultural or geographical record contained within the site attacked’ (p. 153); to ‘transpos[e] the record from the ground to some form, both permanent and available’ (p. 154). These themes are reiterated by Philip Barker with reference to British archaeology in the late 1970s when he argues that excavators should strive for ‘complete objectivity’; their primary goal should be to build a foundation of empirical data that is free of interpretive presuppositions and neutral with respect to the questions archaeologists might later want to take up (1977: 12; Bradley 2015).¹²

On the other hand, the critics of this data-first approach to inquiry¹³ argue that it is untenable and self-defeating; it can never realize the kind of understanding that makes archaeology worth doing. They object, with increasing intensity and sophistication through the post-war years, that it is fundamentally misguided to assume that archaeological ‘facts’ can be recovered independently of any specific purpose or theory. This is to assume a ‘narrow empiricism’, a ‘*simpliste* mechanistic-positivistic philosophy’ as Kluckhohn described it, the limitations of which were already evident by the 1940s; the volume of data recovered had grown dramatically with little commensurate expansion of historical or anthropological understanding (1939; 1940: 46). ‘Passive observation’ is not an option, declared John Bennett (1946: 200); there are no empirical givens that archaeologists can establish as ‘facts’ in a theoretical vacuum. Moreover, this is not a predicament distinctive of archaeology as a social, historical science. Kluckhohn argues that it is impossible to identify, describe and analyse ‘facts’ as evidence without presupposing some set of questions and aligned theoretical presuppositions whether the subject domain is physics or archaeology; ‘no fact has meaning except in the context of a conceptual scheme’ (1940: 47). Kluckhohn concludes:

The alternative is not ... between theory and no theory or a minimum of theory, but between adequate and inadequate theories, and, even more

important, between theories, the postulates and propositions of which are conscious and hence lend themselves to systematic criticism, and theories the premises of which have not been examined even by their formulators. (1939: 330)

Those who purport to collect data neutrally, free of theoretical presuppositions, proceed 'blindly', their empirical as much as their interpretive claims subject to the influence of 'traditional premises and concepts', which Kluckhohn refers to as 'cultural compulsives' (1940: 45, 48).¹⁴

As the challenge of bringing order to rapidly expanding stores of archaeological data became urgent, the focus of debate shifted to questions about the status of archaeological typologies. In the mid-1950s one finds arrayed on one side, with Albert Spaulding, those who insisted that archaeologists can and should establish a stable foundation of statistically defined typological units (Spaulding 1953a) and, on the other, those who rejected the quest for 'ideal-complete-classifications' (Brew 1946: 51) – 'all-purpose, standardized typologies' (Hill and Evans 1972: 237) – as untenable and ill-conceived. Two prominent critics of Spaulding's position, James Ford and John Brew, argued that archaeological systems of classification are 'merely tools, tools of analysis', theoretical constructs that constitute 'the terms in which we think'; they cannot be assumed to capture uniquely and objectively 'real' systems or entities 'inherent in the material' (Brew 1946: 46, 50).¹⁵ It is 'amazingly naïve', Ford argued in a spirited reply to a review by Spaulding (1953b), to think that statistical analysis will reveal 'natural units' (of cultural significance) in archaeological material: is it really plausible that 'nature has provided us with a world filled with packaged facts and truths that may be discovered and digested like Easter eggs hidden on a lawn' (1954: 109)? In the end, Ford argues, archaeologists have no option but to engage in the 'risky business of stacking hypotheses into what may be a shaky structure'; there is no other way forward, 'however uneasy it may make them or however unconscious they may be that they are doing so' (pp. 109, 110).

Drawing out the implications of these arguments a decade later in terms that became a lightning rod for the New Archaeology, Raymond Thompson offered a close analysis of the 'process, limitations, and

potentialities of inference in archaeological research', on the basis of which he concluded that subjective judgement is inescapable, not just in the formulation of interpretive hypotheses but in their evaluation (1958: 30). Influenced by the American pragmatist John Dewey, he distinguished between the 'indicative' aspect of inquiry in which archaeologists explore the features of archaeological traces that suggest their 'inferential possibilities', usually by analogy with similar material encountered in ethnographic contexts, and the 'probative' process by which archaeologists critically scrutinize these initial hunches (pp. 3–4). But despite requiring a step beyond simply accepting an initially plausible interpretation of traces as evidence, Thompson characterized the probative phase as relying on the same methods of inquiry as the indicative; it is a matter of extending the analogical comparison that initially suggests an interpretation, determining whether 'an artifact–behavior correlation similar to the suggested one is a common occurrence in ethnographic reality' (p. 6). And this, Thompson argued, depends on comparisons rooted in typological constructs that are themselves 'abstractions', designed to 'produce groupings of potential cultural significance' (1956). He acknowledged that practitioners who start with different indicative hypotheses will necessarily rely on different 'system[s] for observing cultural data', so they can be expected to draw divergent evaluative conclusions about the 'probity' (the plausibility) of these hypotheses (1958: 8). Hypothesis testing is, therefore, as richly interpretive as hypothesis generation; the probative appraisal of evidential and interpretive claims incorporates an irreducible 'subjective' element (p. 6). He cites half a dozen contemporaries, including Brew and Ford, whose discussions of classification and typology serve to 'remind the reader of the widespread acknowledgment of the role which the subjective element plays in this phase [the probative phase] of archaeological reconstruction' (p. 8), and he concluded on this basis that:

The final judgment of an archaeologist's cultural reconstructions [including typological systems] must therefore be based on an appraisal of his professional competence, and particularly the quality of the subjective contribution to that competence. Our present method of assessing the role of this subjective element by an appraisal of the intellectual honesty of the archaeologist who makes the inference is certainly inadequate. But, there does

not seem to be any practical means of greatly improving the situation despite the insistence of many of the critics of archaeological method. We can only hope for improvements in the methods of measuring the amount of faith we place in an individual's work. (1958: 8)

Such conclusions were widely seen as categorically unacceptable, reason enough to reject out of hand the constructionist arguments that led to them. As Spaulding had objected in the context of the typology debate, such arguments imply that:

... truth is to be determined by some sort of polling of archaeologists, that productivity is doing what other archaeologists do, and that the only purpose of archaeology is to make archaeologists happy. This is simply a specialized version of the 'life is just a game' constellation of ideas, a philosophical position which cannot be tolerated in a scientific context. (1953b: 590)

The crisis debates we have described were largely a North American phenomenon, but Smith's reflections on the 'Limitations of Inference in Archaeology', in the same period in a British context, evoke a strikingly similar set of epistemic anxieties. Although she does not engage the North American debates, Smith makes explicit the terms of a dilemma that underpins them, setting out a starkly drawn opposition between a conception of properly scientific inquiry that requires archaeologists to cleave to the facts, later described as a retreat to 'artifact physics' (DeBoer and Lathrap 1979: 103), and speculative modes of practice that archaeologists will have to embrace if they are intent on addressing anthropological, historical questions about the past but that transgress the limits of scientific inquiry.¹⁶ This dilemma is made all the more intractable by the constructionist critics of a data-first approach who had argued since the late 1930s that the 'no-speculation' horn of the dilemma is untenable. No matter how rigorously archaeologists restrict their focus to empirical questions about the archaeological record, they cannot escape reliance on interpretive assumptions and professional judgement. If the description of the data depends on typological constructs, then Smith's 'element of conjecture' is an inescapable feature not just of ambitious explanatory and interpretive hypotheses about the cultural past, but of evidential claims they use to build and assess these hypotheses.

The most recent advocates of a New Archaeology, the processual archaeologists of the 1970s, refused both horns of this dilemma,

categorically rejecting the pessimistic conclusion that scientific rigour must be sacrificed if anthropological goals are to be pursued. Indeed, in his famous 'fighting articles', Lewis Binford argued that archaeologists should set their sights higher: their goal should be not just to reconstruct past events and conditions of life, but to grasp the underlying cultural processes that explain these descriptive cultural histories. Moreover, he aggressively reaffirmed a commitment to deductive certainty as the epistemic standard to which archaeologists should aspire, even for these most ambitious of explanatory theories. There is some irony in the fact that, in making these strong programmatic claims, Binford took on board the central arguments of the very critics whose conclusions he repudiated; he recognized that archaeological evidence is 'theory-laden' and in this sense a construct, but declared this a reason for epistemic optimism rather than pessimism. The limitations of the archaeological enterprise, he insisted, lie not in the archaeological record itself but in the resources that archaeologists bring to its investigation (1968a: 22).

To realize the full potential of a self-consciously scientific archaeology, Binford argued, the first thing archaeologists must do is abandon the self-limiting 'normative' conception of culture that he associated with 'traditional archaeology'. He thus countered the ontological arguments for pessimism about ever establishing credible knowledge of a distinctively cultural past; if the cultural subject is understood in materialist terms, as a 'material-based organization of behavior' rather than a 'mental phenomenon' and as a system in which all components are causally interdependent (1972: 9),¹⁷ then it should be possible to develop laws of cultural process that support both the reconstruction (retrodiction) and the explanation of past cultural systems, their form, dynamics and trajectories of change. In addition, Binford and a growing number of proponents of the New Archaeology were uncompromising in rejecting any epistemic argument that entails the speculative horn of the dilemma: the conclusion that there is no escape from subjective judgement and arbitrary interpretive convention.¹⁸ This only follows, they argued, if archaeologists confine themselves to an inductive methodology, gathering data and then erecting an edifice of hypotheses about its significance after the fact. Much as Dixon and Wissler had done fifty

years earlier, they made the case for a problem-oriented, hypothesis-testing approach in which interpretive and explanatory hypotheses figure as the point of departure for archaeological inquiry rather than its conclusion:

The generation of inferences regarding the past should not be the end-product of the archaeologist's work ... [I]ndependent means of testing propositions about the past must be developed. Such means must be considerably more rigorous than evaluating an author's presuppositions by judging his professional competence or intellectual honesty. (Binford 1968a: 17)

What distinguished the New Archaeologists of the 1960s and 1970s from these earlier advocates of a 'new' (anthropological, scientific) archaeology was their commitment to the philosophical models of hypothesis testing associated with mid-century logical positivism.¹⁹ If archaeological knowledge of the past was to be 'more than a projection of our ethnographic understanding', Binford argued, archaeologists must implement properly scientific probative procedures in the form of a 'deductive' programme of hypothesis testing; they should derive test implications from clearly specified hypotheses about the cultural past and design their fieldwork and analysis as a test of these implications against 'independent empirical data' (1968a: 17). In early programmatic statements the advocates of a deductive testing programme in archaeology seemed to assume that, on a 'hypothetico-deductive' model of confirmation, positive test outcomes could establish hypotheses with deductive certainty. In fact, the philosophers of science to whom the New Archaeologists appealed for these models had long wrestled with the problem that, even when deductively drawn test implications are empirically confirmed, the support they give to a hypothesis is inductive; in principle, only 'disconfirming' evidence can yield a deductively decisive verdict.²⁰ Moreover, in practice, test evidence decisively refutes a hypothesis only if the 'auxiliary' assumptions mediating the inference from empirical data to test hypothesis are sound; disconfirming results can arise because the auxiliaries are faulty, not because the test hypothesis is wrong. These points were made in archaeological contexts by philosopher Merrilee Salmon (1975), and through the 1970s by a number of New Archaeologists themselves; for example, John Fritz emphasized the role of what he referred to as 'arguments of relevance'

(1972: 140), and Bruce Smith made the case that hypothesis testing is inescapably inductive, offering a sophisticated analysis of the role of bridging arguments that underpin what he described as a 'hypothetico-analog' model of confirmation (1977: 611). Although Binford never abandoned his commitment to deductivist ideals, by 1977 he acknowledged that archaeological data stand as evidence only under interpretation; to assess the test implications of processual hypotheses archaeologically it would be necessary to develop a robust body of middle-range theory capable of establishing the significance of archaeological data as evidence for or against the test hypothesis (Binford 1977). Writing with Sabloff in the early 1980s he explicitly recognized the theory-ladenness of evidence, and Kuhnian paradigm-dependence of inquiry more generally, as an unavoidable fact of scientific life (Binford and Sabloff 1982). By that time the emphasis of his own work, and that of many 'second generation' New Archaeologists, had shifted to experimental and ethnoarchaeological research designed to build the bodies of background knowledge necessary to establish auxiliary hypotheses that could secure the interpretation of archaeological data as evidence.²¹

By the early 1980s virtually every aspect of the self-consciously scientific, positivist New Archaeology research programme had been called into question, from widely divergent perspectives, by external critics who collectively came to be identified as post-processualists (e.g. Tilley 1981). Some took aim at the ontological commitments that underpinned Binford's epistemic optimism, rejecting his eco-materialist conception of the subject domain. They argued that there are many questions of archaeological interest that cannot be answered in these reductive terms; restricting inquiry to these aspects of past cultures drains the subject of anthropological interest. Moreover, Binford gave no very convincing reasons to believe that system-level dynamics and external ecological conditions are the only factors that are explanatorily relevant even if your focus is on understanding large-scale, long-term cultural processes (e.g. papers in Hodder 1987); the normative, ideational elements of the cultural lifeworld that Binford ruled out of bounds for a properly scientific archaeology do have an impact on how human populations interact with their material environments and can powerfully affect the trajectories of

change and development in cultural systems. Others directly challenged the deductivist ambitions of the New Archaeology in terms that revitalize the central lines of argument for a profound epistemic pessimism along the lines of that which had been developed by M. A. Smith and Thompson thirty years earlier. Taking up the objection that hypothetico-deductive testing is not deductive, post-processual critics argued that, no matter how rigorously New Archaeologists pursue a programme of developing middle-range theory, few of the linking principles required to mediate the interpretation of data as evidence could ever be expected to establish evidential claims with deductive certainty (e.g. Shanks and Tilley 1987, Hodder 1992). Even those that fall squarely within the ambit of eco-materialist middle-range theory rarely take the form of universal, bi-conditional ('if-and-only-if') laws of the kind that would be needed to demonstrate that a given type of trace is necessarily and universally the result of specific causal antecedents (events or conditions).

Building on these jointly ontological and epistemic arguments, post-processual critics drew the conclusion that archaeological inference is invariably ampliative; the process of identifying material traces as archaeological, much less using it as evidence to test hypotheses about cultural events and systems, involves drawing conclusions that extend beyond what can be established with certainty by the premises (e.g. Shanks and Hodder 1995). Some then went on to reproduce the dilemmic options that M. A. Smith had delineated and that had framed the crisis debates of the 1930s and 1950s. They objected that even the most resolutely scientific New Archaeologists rely on forms of evidential reasoning that are vulnerable to precisely the kinds of insecurity they had hoped to escape; evidential reasoning incorporates an inescapably subjective 'element of conjecture'. Generalizing this point, some were prepared to argue, for a brief period in the mid-1980s, that if archaeological evidence is inevitably theory-laden it must be admitted that archaeologists simply 'create facts' (Hodder 1983: 6; see also 1984). And if the 'facts' themselves are a theoretical construct, then there are no independent empirical (non-theoretical) grounds for testing overtly theoretical, reconstructive and explanatory claims about the cultural past (Shanks and Tilley 1987: 111). It is a short step from this explicitly social constructionist line of argument

to the relativist conclusions that critics of ‘archaeology by polling’ most fear; as captivating and richly empirical as archaeological narratives may be, they are more about the present than the past and, as such, they should be judged as much on aesthetic and political grounds as on empirical ones.

The final turn in these debates has been frustration and disengagement. The strongest post-processual critiques entail a ‘hyperrelativism’ that ignores the constraints that material traces can impose on archaeological theorizing, and trivializes what archaeologists have achieved by mobilizing these constraints (Trigger 1989); it cannot be a credible account of the evidence with which archaeologists work or of their epistemic practices. The ‘theory wars’, as Matthew Johnson describes them (2010), have died down not because the underlying issues have been resolved but, rather, because the debate has gone underground. ‘Processual-plus’ archaeologists operate in the spirit of the New Archaeology but without restricting the scope of inquiry as Binford had required, and with a more realistic appreciation of the complexity and insecurity of hypothesis evaluation (Hegmon 2003). At the same time, few post-processual archaeologists have maintained an uncompromisingly social constructionist stance of the kind that figured in the polemical debates of the mid-1980s; they quickly qualified their strongest epistemic claims, repudiating the ‘essential irrationality of subjectivism or relativism’ (Shanks and Tilley 1987: 110). But despite a renewed interest in archaeological methodology and in ‘how [archaeologists] reach their conclusions’ (Hodder 1999: 20), few have revisited the assumptions that gave rise to these conclusions.

It is these assumptions – the premises that have framed crisis debates in archaeology for more than a century – that need to be re-examined. The central thesis of this book is that these premises make little sense of actual practice; they misrecognize the capacity of archaeological evidence, construct though it is, to very effectively bite back. In the process, they obscure a wide range of epistemic possibilities that lie between the horns of the dilemma with which successive generations of archaeologists have wrestled.

Two framing assumptions: Why accept the terms of

the dilemma?

To reframe the debate about evidential reasoning in archaeology the first thing to note is that the choice between speculation and ‘artifact physics’ is only a genuine dilemma – these options are only mutually exclusive and exhaustive – if you accept two further presuppositions.

First, you must assume a norm of ‘legitimate inference’ for archaeological inference that sets the bar for epistemic responsibility unattainably high: at deductive certainty. M. A. Smith was by no means alone in equating scientific standards of credibility in inference with deductive entailment; Binford and the early New Archaeologists endorsed this ideal, even though they rejected the conclusion that archaeologists must scale back their ambitions to realize it in practice. And, implicitly, so do the critics who argue that scientific ambitions are unattainable in archaeology when they draw the conclusion that the only alternative to deductive certainty is unconstrained speculation.

To take this further step – to counterpose scientific rigour and speculation as the horns of a classic dilemma – you must also make a second assumption: that the connections between surviving material traces and the antecedent events or conditions that produced them are all equally and extremely tenuous. That is, you must accept some form of Smith’s claim that, in the absence of a truth-preserving relationship of logical entailment, any inference about the cultural past based on archaeological data is an untestable ‘conjecture’ (1955: 3); radical underdetermination is inescapable. To return to our analysis of Smith, this stance presupposes both ontological claims about the nature of the subject domain and epistemic assumptions about the nature of evidential reasoning. Where the ontological claims are concerned, the conclusion that all inferences from material traces are (equally) speculative given the nature of the cultural subject only follows if you are committed to an extreme form of the normative theory of culture described earlier. That is, you must not only reject implausible uniformitarian premises that posit exceptionless ‘if-and-only-if’ laws linking material traces to specific past cultural events or conditions;²² you must also assume that, in the absence of such laws, no regularities hold in any cultural domain that could provide plausible, if less than

certain, support for inferences from surviving traces to the cultural past. You must assume that cultural phenomena are nothing but normative and that, without access to the meanings that animate them, you have no basis for interpreting archaeological data as evidence of a social, cultural past.

The epistemic assumptions necessary to mobilize ‘global’ scepticism about archaeological inference are similarly problematic.²³ You must be prepared to generalize from error and uncertainty in specific instances – recognized in hindsight (like the succession of interpretations of the Red Lady of Paviland) or counterfactually projected (like Smith’s Trobriand Islander and Diogenes examples) – to the conclusion that such error infects all archaeological inference, regardless of its ground. The incongruity here is that, despite a commitment to epistemic caution, such critics are prepared to endorse vastly overextended conclusions about the credibility of any knowledge claim based on archaeological trace evidence. They violate the very inferential standards they invoke, reasoning from premises of limited scope to conclusions about inescapable scepticism. Such premises may show that there is always the possibility of error but, on their own, they do not establish that all inferences in a given domain are uncertain or, more to the point, that they are all equally uncertain.

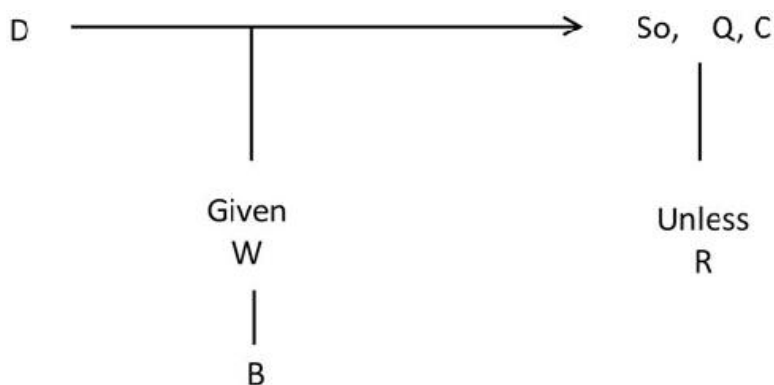
There are good reasons to reject both of these assumptions. We turn now to consider arguments for setting aside deductive validity as the appropriate standard for evidential reasoning in archaeology, and then conclude by making a case for recognizing that cultural subjects can sustain a range of degrees of credibility that lie between the extremes of deductive certainty and unconstrained speculation. In subsequent chapters we work in this in-between space, building a more realistic account of evidential reasoning in terms of a series of case studies. Our aim is, in a sense, to reclaim the paradox we described in the Introduction, in the form of a recognition that, although material traces are undeniably enigmatic, the very ‘brute intransigence’ (Daston 2008: 11) that gives rise to epistemic pessimism also means that they have a remarkable capacity to bear witness to the cultural past in ways that do often subvert our presentist convictions and expand our interpretive horizons.

Beyond deductive validity: the role of inferential warrants in ‘working logic’

Just a few years after M. A. Smith’s (1955) article, a British philosopher, Stephen Toulmin, published a sustained critique of the ways in which philosophical logicians had systematically misrecognized essential elements of practical argumentation by fixating on deductive syllogisms as the model for the justification of ‘claims of interest’. Syllogistic logic is a simplifying idealization that emphasizes formal structure; it captures context-independent conditions of validity for an exceedingly narrow class of analytic arguments, those in which the truth of a conclusion is guaranteed by a universal major premise (Toulmin 1958: 134). By design, they ignore what Toulmin described as the ‘critical function of reason’ (p. 7): the whole range of background knowledge and domain-specific considerations that determine whether a ‘sound argument, a well-grounded or firmly-backed claim’ will ‘stand up to criticism’ in a particular context of action or inquiry (p. 8). Toulmin rejected the ‘superstition’ that arguments should be assessed ‘from outside time’ (pp. 57, 217) and argued forcefully against taking models of formal, truth-preserving entailment as the measure of inferential success. These are, he insisted, wholly inadequate for understanding the ‘working logic’ of domain-specific arguments-in-use (p. 135).

In Toulmin’s constructive account, the cogency and strength of an argument depends not only on the ‘facts’ cited in support of a ‘claim’ but also on ‘warrants’ that license the inference from facts to conclusions (Fig. 1.1).²⁴ These are ‘hypothetical, bridge-like statements’ (p. 98) that establish the relevance of the grounds cited to the putative conclusion; they answer such questions as ‘How did you get there?’ or ‘What have you got to go on?’ Warrants are themselves claims that depend on further substantive arguments; they are not purely formal inference rules, nor are they ‘self-authenticating’, as Toulmin puts it (p. 91), and they are rarely universal in scope. If they have robust enough content to be useful, they are typically context- or domain-specific, subject to appraisal both for their own credibility and with respect to their applicability to the case at hand. In addition to these three fundamental components of an argument – claims, facts

and warrants – Toulmin identifies three additional sets of considerations that arise when an argument is challenged: the ‘backing’ required for warrants when the inferential move from fact to claim is called into question; responses to ‘rebuttals’ that identify exceptions and delimit the scope of an argument; and ‘qualifiers’ that specify the intended strength of an argument. Toulmin’s central point is that the inferential work of warrants should be recognized as critical to the appraisal of substantial arguments; he emphasizes the role of what we will refer to as inferential scaffolding of various kinds, the gap-crossing assumptions, auxiliary hypotheses, background knowledge that constitute middle-range theory in an archaeological context. The secondary elements – qualifiers, backing and rebuttals – signal the pragmatic, dynamic nature of argument-in-use. It is the insistence on raising such questions about the arguments that underpin evidential reasoning that, in our account, characterizes a field such as archaeology when it best approximates scientific ideals. (For an archaeological example of Toulmin’s schema, see [Fig. 1.2.](#))



Main Components

- **Datum:** the facts cited in support of a claim
- **Claim:** the conclusion drawn
- **Warrant:** domain-specific 'bridge-like statements' that license the inferential move from D to C.

Additional Conditions

- **Rebuttal:** conditions of exception, specifying the scope of the claim
- **Qualifiers:** modal operators, specifying the strength of claim (probably, possibly)
- **Backing:** field-specific support for the warranting statements

Figure 1.1 Toulmin’s argument schema: components and conditions. (Source: based on Toulmin 1958: 96–8.)

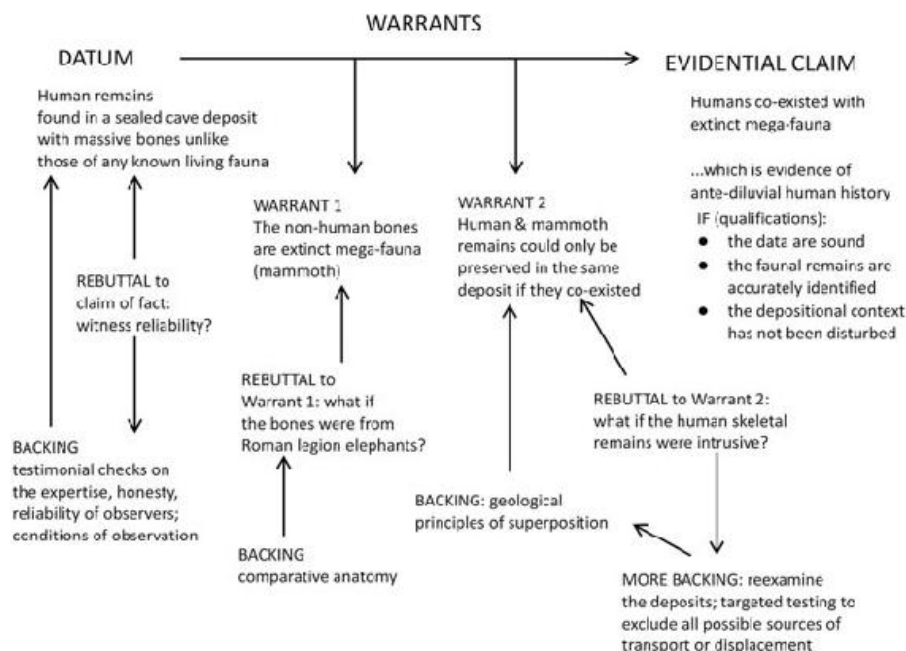


Figure 1.2 A generic archaeological example of an evidential argument laid out according to Toulmin's argument schema (see Fig. 1.1). Toulmin's schema is expanded to recognize rebuttals that arise from questions about the facts (the datum) as well as the warrants, and warrants are understood to include not only domain-specific inference rules (Toulmin 1958: 91) but also domain-specific 'material postulates' (Norton 2003: 648).

At a couple of junctures, Toulmin considers historical and archaeological examples, mainly in contexts where he addresses the threat of a regress opened up by the inescapable fact that the security of deductive entailment must 'in the nature of the case, elude us' (1958: 204). Demands can always be made for additional 'facts' to support a historical or archaeological claim, and challenges can always be brought against the warrants that bridge the gulf between them. No matter how much evidence we amass, 'the ambition of entailing truths about the past will remain as far off as ever', at which point the slide into scepticism opens up for those who embrace the ideal of deductive entailment: it seems unavoidable that 'the caution with which we properly receive the more tentative claims of archaeology must be extended ... to matters about which we had previously experienced no serious doubt' (p. 204). Toulmin acknowledges that, judged against deductive standards, the 'substantial arguments' on which archaeologists and historians rely are 'irreparably loose and lacking in rigour'; the grounds they can claim

are 'never entirely compulsive or ineluctable in the way that logical necessity can be' (p. 142). Here he captures exactly the conundrum faced by successive generations of 'new' archaeologists and their constructionist critics; if deductive certainty is the ideal, even the most cautiously framed arguments for claims about the cultural past just 'never come up to standard' (p. 142), and no amount of additional evidence that supplements their explicitly cited grounds or reinforces implicit warrants will resolve this 'epistemological quandary'.

Toulmin's recommendation is to 'abandon the ideal of analytic argument' and the standard of deductive certainty that comes with it (p. 213): give up the quixotic quest for 'God's-eye justification', 'justification-for-good-and-for all' (pp. 218–19), the absolutist compulsion to seek 'substantial arguments [that] do not need redeeming' (p. 214). He concludes that:

The proper course for epistemology is neither to embrace nor to armour oneself against scepticism, but to moderate one's ambitions – demanding of arguments and claims to knowledge in any field not that they shall measure up against analytic standards but, more realistically, that they shall achieve whatever sort of cogency or well-foundedness can relevantly be asked for in that field. (1958: 229)

This requires that, rather than seek self-warranting empirical foundations or formal guarantees of truth that apply across the board, it is crucial to adopt the 'eye of a naturalist' (p. 235). Toulmin advocates a 'confessedly empirical', comparative and historical approach to the study of logic-in-use (p. 221): recognize that credibility is an 'intra-field, not an inter-field notion' (p. 235), identify 'the actual forms of argument current in any field' (p. 237), and take seriously the historical and contextual dynamism of field-specific forms of argumentation. Logic-in-use evolves; it is 'all of a piece' with the substantive concerns, resources and accomplishments of the field in which it has taken shape. Rather than fixate on the threat of wholesale scepticism, apportion epistemic suspicion to context and purpose. Ask 'what sorts of thing one can relevantly take into account when facing actual problems in different fields' (p. 222); address specific risks of error and alternative conclusions that are realistic threats to the soundness of an argument; and recognize forms of support that are 'for practical purposes un rebuttable' (p. 230) – in our

terms, scaffolding that is fit for purpose. The stopping point will not be absolute, self-warranting empirical foundations but those presuppositions that are ‘more unreasonable to doubt than to believe’ (p. 230), none of which will be immune to critical scrutiny. By extension, Toulmin’s analysis suggests that success in pushing a research programme forward will depend on a complex balance between accepting the scaffolding that makes it possible to probe a focal set of questions as unproblematic for current purposes, and selectively subjecting components of this scaffolding to critical scrutiny as relevant knowledge grows, both exposing its limitations in scope and credibility and raising new questions. What matters is not approximation to an absolute ideal of certainty but the ‘trustworthiness’, the ‘reliability’ (p. 66), of evidential reasoning as a basis for action, whether in an everyday practical sense of the kind that chiefly concerns Toulmin, or in the context of systematic empirical inquiry.

Toulmin’s call for attention to field- and domain-specific standards of argumentation resonates in striking ways with David Clarke’s later, prescient argument that archaeologists should abandon the comfort of an illusory ‘innocence’ about their norms of practice and attend to field-specific ‘patterns of archaeological reasoning ... without making any unwarranted assumptions that the principles of logic and explanation are simple universals which may be transferred from one discipline and level to another’ (1973: 15). In developing a contemporary account of archaeological thinking, Orser (2015: 8–9) likewise calls for an analysis of actual practice and in this he draws inspiration from an analysis of practical reasoning by British philosopher Susan Stebbing, *Thinking to Some Purpose* (1939), that raises many of the same issues as Toulmin did two decades before he published *The Uses of Argument*, but with a focus on public, political discourse. She argues that ‘thinking’ often fails not because it violates formal canons of logic but because we are missing relevant information, or lack sophistication in assessing the evidence that is presented and in detecting rhetorical distortion, all context-specific considerations that Toulmin foregrounds in his account of ‘substantial argument’.²⁵

Such an approach is also prominent in American pragmatism as

developed by Dewey, whose influence is reflected, in an archaeological context, in R. H. Thompson's arguments for recognizing the relevance of professional competence and the question-specificity of inferential warrants (1956). There is much to regret in the fact that American archaeologists could not see Thompson's Deweyan pragmatism as anything but a recommendation that archaeologists pander to the authority of self-styled experts and disciplinary conventions, and that Toulmin did not get an uptake in archaeology in the 1950s when Smith outlined the Diogenes problem and argued that, because archaeologists' inferences from and about evidence cannot but fall short of deductive certainty, they should abandon any ambition of understanding the cultural past.

That said, archaeology is by no means the only context in which the perennial attraction of deductivist ideals has been difficult to dispel. It is relatively recently that philosophers of science have taken up the challenges of understanding actual rather than idealized research practice that Toulmin had articulated in the 1950s.²⁶ In a contemporary classic on inductive inference in the sciences, John Norton argues that we should abandon the quest for universal inference schemas that spell out formal, context-independent conditions for inductive success on the model of deductive logic (2003). Unlike deductive arguments, what determines the credibility and the strength of inductive arguments is their content, not their form; they depend on 'material postulates' about causal relations, properties and regularities that function as warrants for inference within a given domain (p. 648). This last is crucial: given that 'the world is simply not uniform in all but a few specially selected aspects' (p. 651), Norton argues that the 'portability' of these postulates is limited; they underwrite inference only within fields where the regularities and causal dynamics they capture can be shown to obtain (p. 663). It should not be surprising that the 'extraordinary success of science at learning about our world through inductive inquiry' (p. 648) is primarily a function of advances in domain-specific knowledge that provides a richer, more robust array of substantive inference-grounding postulates, not the invention of new formal inference schemas. Norton's advice is to adopt a 'material theory of induction' and give up the quest for a universal standard of inferential adequacy

(p. 669).

These themes have been taken up with reference specifically to reasoning from evidence by Julian Reiss, who is similarly suspicious of any appeal to universal, context-independent schemas for judging the reliability of inferences that constitute evidential claims. He makes the case for a 'pragmatist theory of evidence' (2015: 341) which focuses attention on the role of substantive, context-specific background knowledge that functions like Norton's material postulates, as causal warrants for evidential inference in all but the most rarefied of circumstances. Of particular relevance here is Reiss's argument that the classic 'hypothetico-deductive' theory of confirmation, so influential in archaeology, offers an insight worth salvaging if disentangled from deductivist ideals. In his account, empirical data constitute evidence that supports a hypothesis if they are what 'we would expect to hold if the hypotheses were true' (p. 346), given background knowledge about the composition and the causal dynamics of a specified domain of inquiry. This direct support for a test hypothesis is invariably inductive and comes in degrees; rarely can it settle the case for a unique conclusion. Moreover, confirmation of this kind is irreducibly comparative; the credibility afforded by direct support must be supplemented by indirect support in the form of evidence that rules out alternative hypotheses which, if true, could also explain the evidence that provides direct support (pp. 347–8). This 'hypothetico-contextualist' account draws attention to just the kinds of domain-specific knowledge that Norton cites as critical to the credibility of inductive inference, and just the kinds of context and purpose-specific 'warrants' for inference that are central to Toulmin's account of practical argument. We count these among the elements of the scaffolding that are required to build and assess evidential claims in archaeology.

What all these advocates for 'logic-in-use' call into question is a fundamental mistake that, we have argued, drives crisis debate in archaeology: the substitution of idealized logic, in the form of universal inference schemas like deductivist ideals, for close analysis of the evolving, field-specific working logic by which archaeologists actually appraise arguments. There are good reasons to join them in rejecting the first of the two premises that generate crisis debates

about the evidential limitations of archaeological inquiry.

Degrees of credibility

The actual practice of building and continuously rebuilding the warrants that underpin evidential arguments in archaeology reflects two sets of intuitions that call into question the second assumption driving the crisis debates, namely that all proxy evidence is, by its nature, equally and radically insecure.

The first intuition is that archaeologists deal with many different kinds of phenomena in the context of studying the 'cultural' past, some of which are constrained and deterministic while others are highly contingent, normative and conventional. Some biophysical dimensions of human lives support relatively stable and reliably projectable regularities that are to varying degrees 'portable' across cultural contexts, while others are highly specific to a particular social, ideational context. No single, categorical claim can be made about the ontological status of the cultural subject as such. It is a mistake to assume that the insecurity and open-endedness of inferences about the symbolic significance of the ochre staining distinctive of the Red Lady of Paviland is characteristic of all the evidential claims that can be made about her – the inferences by which her skeletal remains were dated, her sex determined, and her stratigraphic association with ivory artefacts was established – just because they are all 'theory-laden'. It is, likewise, a mistake to assume that symbolic interpretations lack credibility altogether and to declare investigation of these dimensions of the cultural past off-limits because inferences about them typically do not rise to the level of security that can be realized with respect to physical dating and biological analysis.²⁷ The goal of grasping symbolic significance may seem quixotic, but it is often possible to distinguish between more and less plausible attributions of meaning even if both direct and indirect evidence underdetermines the case for any one interpretation. Evidential claims of all these kinds are contingent and heavily scaffolded, but the warrants that can be invoked to underwrite them differ with respect to security and the degree to which they can be transferred from one context to another, a function of the nature of

symbolism and of human skeletal remains as well as the state of the (evolving) background knowledge that grounds them. What matters is to be clear about the backing, scope and strength of the claims made in these different cases. In short, the inferences linking surviving traces to inferred antecedent events are differentially insecure; few (if any) rise to the level of deductive certainty, but they are not all irreducibly or equally speculative, for ontological as well as epistemic reasons.

The second intuition implicit in practice is that the theory-dependence of archaeological evidence does not in any necessary or comprehensive sense entail vicious circularity. The background knowledge, assumptions and expectations that play a role in interpreting data as evidence – the ‘lading’ theory – only predetermine what archaeologists will find or recognize as evidence if there is a hyper-integration of inferential warrants and test hypotheses. That is, a compromising circularity only follows if the middle-range theory that archaeologists rely on to interpret data as evidence is the same as, or presupposes, the hypothesis this evidence is used to support or to test. This is, of course, always a risk, but it is not a given. Even in what seem like worst-case scenarios, where auxiliary and test hypotheses are both components of an overarching theory, philosopher Clark Glymour makes a compelling case that very specific conditions of interdependence must obtain between the components of a theory to create a situation in which the auxiliaries determine, in advance, that any test data generated by experiment or observation will prove to be evidence that conforms to expectations. His account of ‘bootstrap’ confirmation,²⁸ in fields as diverse as psychoanalysis, experimental psychology and astronomy, shows that a degree of autonomy can be realized between the elements of a theory that are the subject of investigation (the test hypothesis) and those that are used to interpret data as evidence (the auxiliaries) such that the outcome of observation or experimentation can disconfirm a test hypothesis even if it is drawn from the same source theory as the auxiliary hypotheses (1980).

In practice, however, rarely does an archaeological theory provide not only test hypotheses but all the linking principles and background knowledge necessary to establish evidential claims that are relevant

for evaluating these hypotheses. A characteristic feature of the warrants on which archaeologists rely is that, given the ontological diversity of the subject domain, they represent an enormously heterogeneous array of background knowledge, pitched at different levels of generality and established with widely varying degrees of security. They include abstract theoretical presuppositions about the nature of the cultural subject – for example, Clarke’s ‘controlling models’ (1972a: 5–10); the broad paradigm-defining commitments associated with Taylor’s ‘normative’ theory and Binford’s sharply contrasting ‘ecosystem’ theory of culture – as well as closely specified empirical claims about specific aspects of the subject domain: the geology of stratigraphic superposition, the human physiology of sex differences, the physical chemistry of carbon isotopes, the ethno-history of mortuary practices as well as a growing roster of archaeological comparanda.²⁹ In short, archaeological data are ‘laden’ by a great many different kinds of ‘theory’; as inferential warrants, the kinds of backing these require to underwrite evidential claims in archaeology vary dramatically from case to case, and so too does their credibility and scope of application. This means that no unitary judgement can be made about the tenuousness, or the strength, of the inferences that underpin evidential claims, linking material traces to the cultural antecedents of archaeological interest.

Changing the question

Taking seriously these intuitions-in-practice means changing the question. The challenge, in assessing evidential reasoning in archaeology, is not to defend or rebut uncompromising pessimism or optimism about the credibility of archaeological evidence. Rather, it is to delineate the range of strategies by which archaeologists build and adjudicate evidential arguments that always fall short of ideals of certainty but none the less achieve varying degrees of credibility calibrated to context and use. In what follows we will explore three implications that follow from these arguments for working in this space between the horns of Smith’s dilemma.

First, it follows from the argument developed here that much of the action in archaeological inquiry is off-stage, a point made as strongly

by constructionists such as Thompson as by the sharpest of his New Archaeology critics. What makes it possible to identify and recover material traces, and then put them to work as archaeological evidence, is not only the labour and luck of ‘capturing’ new data; it is also, and crucially, a matter of building inferential scaffolding. Work on the warrant-side of the inferential equation involves recruiting, or developing, the background knowledge and technical resources necessary to license evidential reasoning about specific types of material trace, in relation to particular archaeological claims. This is an ongoing process of assessing the credibility and security of background knowledge considered in its own terms, and of determining the scope of its applicability to archaeological problems.

Second, no matter how secure the warrants are that establish the evidential significance of a particular material trace, rarely is evidential reasoning in archaeology a matter of mobilizing a single line of evidence. This is in part a function of the uncertainty and complexity of proxy evidence; there are few silver bullets in the arsenal of archaeological evidence that could convincingly clinch an interpretive or explanatory claim all on their own. In a more positive sense the need to recruit multiple lines of evidence is a function of the heterogeneity of archaeological data and of the ‘material postulates’ that warrant their interpretation as evidence. Archaeologists routinely exploit this heterogeneity, trading on the independence of the causal pathways by which the events or conditions of archaeological interest produce diverse types of trace, and the epistemic independence between the methods and bodies of background knowledge required to underwrite the inferences about their significance as evidence. This disunity of resources – empirical and conceptual – makes possible strategies of triangulation by which ‘cables’ of inference are built; each strand is to varying degrees insecure taken on its own but, when the strands converge, their capacity to constrain one another is a source of evidential robustness that establishes much stronger support for an archaeological claim than the appraisal of security in individual lines of evidence could provide.

Finally, the third implication is that archaeological inquiry is necessarily a matter of bootstrapping in a generic sense.³⁰ In the absence of ‘infallible foundations’ (Chang 2004: 231), archaeologists

necessarily build provisional empirical foundations as they go. Inferential warrants are one component of what we characterize as the scaffolding necessary for doing this, where we conceive of scaffolding in a sense suggested by William Wimsatt.³¹ His theoretical ‘architecture’ for understanding cultural change, including change and innovation in science, highlights the contingent but path-dependent processes by which earlier innovations – in the development of competencies, social relations and institutional structures as well as knowledge – shape what comes later; entrenched as infrastructure, they make later innovations possible but at the same time they delimit what form these can take (Wimsatt 2014).³² In this spirit, we expand the roster of resources that makes evidential reasoning possible to include not just the conceptual content of explicit warrants but also implicit assumptions, tacit knowledge, embodied skills, and all the social, material, technical, economic and institutional conditions that play a role in mobilizing these empirical and conceptual resources. Together these serve to stabilize the inferential scaffolding archaeologists rely on and also, at critical moments, to destabilize it, in the manner suggested by Chang (2004). Foregrounding the role of scaffolding for evidence brings into sharp focus the dynamism of these provisional foundations. They are domain- and purpose-specific, built to enable archaeologists to address particular questions, so they are themselves subject to continuous refinement and extension as inquiry proceeds, and to supplement or replacement as new questions arise. This process of ‘epistemic iteration’, as Chang describes it, is both conservative in the sense of entrenchment, and progressive in so far as it continuously opens up new possibilities for inquiry – including possibilities for discovering where error lies in entrenched theories and trusted scaffolding.

Notes

1

Margaret Smith studied at Oxford with Christopher Hawkes (Professor of European Archaeology), and served as his Departmental Assistant in the mid-1950s. She later published a series of papers on Bronze Age hoards, some of

them co-authored with Hawkes and with A. E. Blin-Stoyle, under her married name, M. A. Brown (Diaz-Andreu 2012: 277–8).

2

For an excellent account of deductive logic in an archaeological context, see Orser on ‘deducing’ (2015: 70–3). Deductive logic focuses on the structure of arguments – the relations of entailment between the premises and conclusions of an argument – rather than on questions about the content of an argument. If an argument form is deductively ‘valid’, its structure is such that the conclusions must be true if the premises are true. It is a separate question whether an argument is ‘sound’, meaning that its premises are actually true, so that conclusions deductively drawn from them – conclusions which do not add any content to that which is established by the premises – can be accepted as true. Deductive inference is typically contrasted to various types of ampliative inference (e.g. inductive, analogical, abductive) in which the premises cited may provide support for the conclusions drawn, but, even if true, they do not guarantee that the conclusions are true.

3

Diogenes was a fourth-century BCE philosopher associated with the Cynics, famous for carrying to an extreme their renunciation of worldly wealth and ambition. He is said to have lived for a period in a large tub in a public square in Athens.

4

Ontological questions concern what exists, the nature of being or reality and, by extension, the status of the categories and concepts in terms of which we describe and explain things that we take to exist. Epistemic questions have to do with the nature, limits and scope of knowledge, and the norms of justification by which we establish knowledge claims. As we use the term here, methodology refers to research strategy, a set of mid-level questions that lie between the abstract concerns of epistemologists and more localized questions of research method or technique.

5

This account of crisis debates about evidence in archaeology summarizes the historical background presented in the first three chapters of *Thinking from Things* (2002b), and develops a line of argument proposed in Wylie (2011a).

6

Pessimism about the prospects for securing credible knowledge about long-past events is a dominant theme in philosophy of the historical sciences. See, for example, Turner’s analysis of the limitations of palaeontology (2007), and

Currie's account of the dynamics of debate between epistemic pessimists and optimists in the philosophy of geology, palaeontology and cosmology (2014a, 2014b).

7

We invoke, here, terminology used by both philosophers of science and archaeologists to refer to these connecting links in inferences from empirical premises. We will characterize them below as 'warranting assumptions' or, more broadly, as a component of the scaffolding on which archaeologists rely to identify and mobilize data as evidence. See Kosso (1991) and Raab and Goodyear (1984) for assessments of Binford's use of 'middle-range theory' to capture this mediating relation. The terms 'linking principles' and 'gap-crossers' are more generic references to any type of background knowledge required to identify, interpret and systematize archaeological data as evidence.

8

'Underdetermination' refers to cases where the available evidence can support more than one theory or hypothesis; it does not 'determine' a unique theoretical conclusion.

9

The philosopher of science N. R. Hanson is credited with introducing the term 'theory laden' in *Patterns of Discovery* (1958). He describes observation as a "theory-laden" undertaking' in the sense that 'observation of x is shaped by prior knowledge of x' (p. 19); and he argues that causal claims should likewise be understood as 'theory-loaded from beginning to end': what we refer to as causes 'are not simple, tangible links in the chain of sense experience, but rather details in an intricate pattern of concepts' (p. 54).

10

We draw on Hacking's account of constructionism here (1999: 5–6, 12–14). Rather than offering a definition, he recommends asking what the point is of constructionist arguments. And in answer to that question he argues that their point is, characteristically, to show that a claim, a state of affairs, or a norm we had assumed to be a given, invariant or inevitable is, in fact, contingent. He is particularly concerned with 'social' constructionist arguments which rely on historical genealogies to demonstrate that a norm such as objectivity, for example, has evolved over time taking very different forms in different social, institutional contexts; Daston and Galison (2007) develop just such an analysis (see Chapter 2 for a discussion of their account of 'aperspectival' objectivity). We use the term more generically; social, institutional conditions play a critical role in the 'construction' of what counts as archaeological evidence, but not to the exclusion of material, empirical and conceptual factors.

11

By ‘vicious circularity’ what we have in mind is the kind of worst-case scenario in which the background knowledge and assumptions archaeologists rely on to interpret their data as evidence guarantee that this evidence will fit their preferred hypotheses; the evidence cannot function as a neutral arbiter of the credibility of the theories they test against it (Glymour 1980: 107).

12

We cite Barker in this connection because he is widely identified as an advocate of a purely empirical approach. However, as we will argue in Chapter 2, Barker’s views were more nuanced. Although he did endorse a ‘data first’ research strategy, he also acknowledged the force of arguments, like those developed in the late 1940s by Kluckhohn, that even the most empirical of observations are theory-laden. We also note that Taylor’s arguments are similarly complex, inasmuch as he is sharply critical of colleagues who have lost sight of broader, historical and anthropological aims; at some junctures he argues that a preoccupation with data collection and description as ‘an end in itself’ (1948: 46) is not just a failure of ambition but risks foreclosing investigative possibilities (p. 113; see Wylie 2002a: 53–5).

13

One of us has used the term ‘sequent stage’, drawn from Kluckhohn (1940: 49), to characterize this recurrent theme in crisis debates: that archaeologists should first gather the data, and defer interpretive or explanatory theorizing about it to later stages of inquiry (Wylie 2002b: 39–41).

14

The conflict between these positions is stated in especially memorable terms in the 1930s. Advocating a sequent stage approach, Strong insisted that, as a ‘youthful science’, archaeology should be centrally concerned with ‘the accumulation of essential data which in many cases are disappearing with alarming rapidity’; it is the better part of wisdom to leave the interpretation of these data to ‘a future time of greater leisure and fullness of data’ (Strong 1936: 365). Two years later, Steward and Setzler published the following rejoinder:

When taxonomy and history are thus complete [when every possible element of culture will have been placed in time and space ... the invention, diffusion, mutation, and association of elements will have been determined], shall we cease our labors and hope that the future Darwin of Anthropology will interpret the great historical scheme that will have been erected? (Steward and Setzler 1938: 3)

15

These are classic examples of what we will refer to as constructionist arguments in an archaeological context. Brew goes on to argue that ‘cultures are not “discovered,” “types” are not “found”’, but rather, the archaeologist ‘*makes it* [the type category] and *puts* the object in it’ (1946: 46). Classification systems have an irreducibly ‘pragmatic element’ as well as being ‘empirical and rational’ (p. 48); no matter how useful and foundational they may be to the description of archaeological material, it is a fundamental mistake to assume that they ‘exist in objective reality’ (p. 50) and to treat them as ‘ideal-complete-classifications’ (p. 51).

16

In their discussion two decades later, DeBoer and Lathrap describe this as ‘the familiar quandary of choosing between a significant pursuit based on a faulty method or one which is methodologically sound but trivial in purpose’; those who opt for the epistemically safe course limit themselves to a kind of ‘artifact physics’, while those who embrace anthropological goals must rely on an ‘overextended uniformitarianism in which past cultural behavior is “read” from our knowledge of present cultural behavior’ (DeBoer and Lathrap 1979: 103). Similar analyses were offered by Klejn, who describes this dilemma arising from a ‘skeptical tradition’ that effectively ‘condemn[ed] archaeology ... to a choice between collecting (the “new antiquarianism”) and “subjective guesses”’ (1977: 3–5).

17

Binford rejected any preoccupation with ‘paleopsychology’ as irrelevant to the goals of explaining cultural phenomena in processual terms. In his account the appropriate subject for a self-consciously scientific archaeology – one that seeks explanatory understanding – is culture conceived as the ‘extrasomatic means of adaptation for the human organism’ (1962: 218).

18

Binford was particularly withering in his critique of Thompson’s pragmatism, invoking Spaulding’s critique of Ford (1968b: 270).

19

Binford relied primarily on Hempel’s *Aspects of Scientific Explanation* (1965). Watson, LeBlanc and Redman (1971) developed this account in much more detail, drawing on Rudner (1966), contributors to a classic anthology of the period (Feigl and Brodbeck 1953), as well as an introductory philosophy of science textbook by Hempel (1966).

20

The reason for this is that, if the premises of a deductive argument entail a conclusion that is found to be false, then, given the truth-preserving nature of such arguments, one or both of the premises must also be false. But establishing that the conclusion is true does not, on its own, confirm the premises; it is always possible that the conclusion (in this case, the test implication derived from the hypothesis under consideration) could be entailed by other premises (an alternative hypothesis), or that it is trivially confirming (just not disconfirming). The challenge taken up by philosophers of science sympathetic to some form of hypothetico-deductive confirmation theory has been to develop criteria for determining when confirming evidence is relevant and discerning in providing support for a specific test hypothesis (see, e.g., Norton 2005). These were among the reasons why Popper had maintained, since the interwar period, that justificationist theories are untenable. He endorsed a deductive standard of certainty and advocated a falsificationist account of theory testing on the grounds that this standard can only be met by evidence that decisively refutes ‘bold conjectures’; see his discussion of the genesis of his account (1963: 33) and the preface to the first edition (1934) of *The Logic of Scientific Discovery* (1959). By contrast, Hempel maintained a commitment to confirmationist models but increasingly focused on inductive and statistical models of confirmation. For a recent appraisal of the viability of a modified version of Hempel’s account, see Reiss’s proposal (2015) of a ‘hypothetico-contextualist’ theory of evidence, discussed below.

21

See, for example, Binford’s *Bones* (1981) and collections of essays on ethnoarchaeology (Gould 1978, Kramer 1979), and, in a British context, Coles’s *Archaeology by Experiment* (1973) and Orme’s *Anthropology for Archaeologists* (1981).

22

These are premises that, if true, would guarantee the truth of the conclusion that, given the type of trace observed, specific antecedent events or conditions must have obtained. They turn the inference of evidential significance into a deductive argument.

23

Global, radical or wholesale scepticism is typically contrasted with local scepticism, as arguments that lead to general conclusions about the possibility of knowledge as such rather than questioning the adequacy of specific knowledge claims or the possibility of knowledge in a specific domain. Methodological scepticism is likewise contrasted with these forms of philosophical scepticism; it is a critical stance characterized by a commitment to subject all knowledge claims to critical scrutiny.

24

We note that Binford discusses ‘warranting arguments’ at several junctures; for example, in *Working at Archaeology* (1983) where he discusses their role in establishing the relevance of interpretive principles to particular archaeological arguments (p. 13) and, more systematically, in ‘Objectivity – Explanation – Archaeology – 1981’ (reprinted in Binford 1983). In ‘Objectivity’ he describes the status of knowledge claims and the ways we typically defend them in terms that are strikingly similar to Toulmin’s account of logic in use. They are, he says, ‘arguments advanced that tend to *warrant to others* the beliefs one has about the world’, drawing on premises that are typically implicit but widely held in a given ‘intellectual context’ or paradigm (p. 46). Although Binford refers to Toulmin in other connections citing later work, we have found no direct references to the conception of ‘warrants’ Toulmin developed in *The Uses of Argument* (1958), and in ‘Objectivity’ he reaffirms his commitment to the demanding ideals of epistemic security that Toulmin calls into question. Binford is emphatic that, while warranting arguments ‘may be sufficient to justify serious consideration’ of a claim, on their own they are ‘unsatisfactory epistemic criteria for any endeavor that seeks to evaluate cultural forms of thought’ (p. 47).

25

As Stebbing puts the point of her analysis with respect to the limitations of deductive (syllogistic) argument schemas:

To think logically is to think relevantly to the purpose that initiated the thinking; all effective thinking is directed to an end. To neglect relevant considerations would entail failure to achieve that end. There is prevalent a strange misconception with regard to the nature of logic ... not all sound arguments are syllogistic. (1939: 10–11)

26

In fact, it is striking that, despite considerable resonance with Toulmin’s practice-oriented approach, few directly engage with his arguments for attending to logic-in-use.

27

Indeed, the seemingly secure warrants for inference drawn from nuclear physics, material science and the life sciences are often quite contentious in application to archaeological problems, as we will discuss in Chapter 4.

28

Glymour (1980) offers a technical account of what he calls ‘bootstrap

confirmation' in response to well rehearsed problems with the hypothetico-deductive account: that it is too indiscriminate in allowing any evidence that confirms a test implication derived from a theory to support the theory as a whole. He shows how evidence can bear on a specific component of a theory given the internal structure of relations between its constituent elements. This is the argument we invoke here, but elsewhere we use the term 'bootstrapping' in a less technical sense to refer to the ongoing processes of 'epistemic iteration', as Chang describes them (2004: 44–6), by which researchers revise and refine the technical, inferential scaffolding on which their investigative practices in a given field depend. Chang notes that 'the process of self-improvement arising from the dialectic between respect and progress might be called bootstrapping', but he avoids the term because, in philosophical contexts, it is so closely associated with Glymour's account which, as he puts it, 'indicates a particular mode of theory testing, rather than a more substantial process of knowledge creation' (p. 45).

29

That is to say, the status of these claims as warrants is functional; what makes them middle-range theory is not a matter of specific content or level of abstraction but, rather, the role they play as 'material postulates' that license inference in an evidential argument.

30

That is, not in the strict intra-theoretic sense of bootstrap confirmation developed by Glymour (1980).

31

We draw on an account of 'robustness' reasoning developed by Wimsatt (1981, 2012) in Chapter 4 when we discuss the use archaeologists make of multiple lines of evidence and in this connection we refer to a related philosophical discussion of scaffolding due to Norton (2013). He describes the evidential reasoning typical of the sciences as 'highly connected, massively tangled' systems of inductive support that draw on a diverse epistemic resources.

32

Wimsatt reframes the constitutive elements of evolutionary theory in terms that, he argues, can usefully be extended to cultural change. What we draw on here is his account of scaffolding as 'structure-like dynamical interactions' among 'performing individuals' that, once in place, function as a means by which other structures and competencies can be developed (2014: 81), and his account of how scaffolding that becomes entrenched creates infrastructure of various kinds that maintains entrenched elements and canalizes subsequent innovation (pp. 83–8). One example he uses to illustrate this is the development in the mid-

nineteenth century of the standardized parts and mass production – the ‘American manufacturing system’ – that became the cornerstone for the Industrial Revolution. This turned on innovations that he describes as initially a response to a military need for muskets that could be repaired in the field. This need was met by developing interchangeable parts which, in turn, required a reconfiguration of the process of production: the decomposition of tasks and the development of machine tools capable of calibrating components across production facilities. As these methods were taken up elsewhere, they resulted in the entrenchment of specialized design and production expertise alongside the deskilling of labour on the production line, and the standardization of parts across industries as well as a host of other downstream effects including, for example, marketing and delivery systems predicated on modularization, and the necessity of building in back-compatibility as technology evolved (pp. 93–101).

Archaeological Fieldwork: Scaffolding in Practice

The paradoxes of material evidence that dominate crisis debates in archaeology are by no means just an abstract philosophical preoccupation; they are clearly manifest in field practice. The conviction that archaeology, *qua* archaeology, should be approached as a technical practice of data recovery – that the priority of responsible fieldwork should be to build a secure foundation of empirical evidence that is neutral with respect to the problems it may be used to address, unadulterated by the ‘distorting filter of human interpretation’ (Daston 2008: 13) – is compelling for many. And yet, on closer examination, even those most closely associated with this ideal of ‘aperspectival’ objectivity clearly articulate their field practice with ‘theory’ and approach it with a perspective-specific sense of purpose. The insight that orienting questions and theoretical presuppositions configure excavation, data collection and recording ‘all the way down’ – that interpretation ‘occurs at the trowel’s edge’ (Hodder 1997) – is reinforced when we consider pivotal innovations in field practice that have transformed what archaeologists are positioned to recognize as archaeological data and what they count as archaeological evidence. The history of fieldwork is a history of learning to ‘see’ (Bradley 1997), a process that depends on the development of scaffolding in the form of technical expertise and community norms of practice which are internalized by individual practitioners as embodied skills and tacit knowledge, and externalized in the material and institutional conditions that make possible the exercise, and the transmission, of these skills and this knowledge. Seeing in this sense is a conceptual as well as perceptual attunement that reflects shared assumptions about the nature of the subject and the purposes of inquiry (Lucas 2001a: 8–9, 18–63). It should not be surprising that different originating problems and ‘pre-understandings’

give rise to divergent fieldwork traditions; cross-field comparisons as well as genealogies throw into relief the conventional, purpose-specific nature of field practice.

In this chapter we focus on excavation practices, although this is just one mode of fieldwork by which archaeologists recover and record the primary data on which they base evidential claims. In this connection we take up the arguments put forward by constructionist critics since the 1930s that archaeological field practices – including excavation strategies and aligned recording practices – are a set of solutions to specific problems, however opaque these may be to practitioners once a canon of conventions has come to define what it means to do fieldwork ‘right’. We explore the process of building the scaffolding necessary to establish sustained, coordinated fieldwork traditions that make possible the capture of archaeological data and the production of archaeological evidence. Once they are entrenched, innovative techniques that have creatively and iteratively defined what it is to do archaeology become norms of practice; they enable major advances in inquiry, but they can also profoundly limit inquiry long after the very evidence they were instrumental in generating has undermined the assumptions and redirected the questions that set the research programme in motion. We close with a consideration of strategies for holding scaffolding accountable – to its archaeological purpose and to a growing body of material evidence – when it risks becoming ossified as unreflective convention.

Seeing and learning to see

A tale of two ‘technicians’

Consider two towering influences in British field archaeology, pioneers of systematic excavation and data collection: General Augustus Lane Fox Pitt Rivers, famous for his excavations of later prehistoric sites on Cranborne Chase, Dorset (UK), in the last two decades of the nineteenth century; and Philip Barker, an admirer of Pitt Rivers who is often portrayed as the ‘arch-exponent’ of a field archaeology that proceeds ‘without asking questions, in order “to see what’s there”’ (Carver 2009: 27). Both are widely regarded as

technicians rather than theorists whose fieldwork practices are understood to embody a separation of empirical data recovery from interpretation that has come to define 'modern' archaeology.

Pitt Rivers's record of careful observation, detailed recording and model publication has taken on the status of discipline-defining legend; he was recognized in the 1930s by Grahame Clark as the 'first scientific British archaeologist' (1934: 414) and canonized by Sir Mortimer Wheeler (e.g. 1954) as the founding father of field archaeology. But the perception that he was chiefly a tactician misses significant complexities in the practice and legacy of this historic figure (Bowden 1991). Although Wheeler saw himself as a disciple of Pitt Rivers, citing this founding father's excavation practice as the inspiration for his own widely influential stratigraphic method (Bowden 1999: 133), Pitt Rivers typically excavated in spits of arbitrary depth (instead of stratigraphic layers), with the exposed profiles recorded in widely varying detail, sometimes in terms of 'average' sections (Barrett and Bradley 1978, Barrett, Bradley and Green 1991). His primary interest was not to understand the depositional history of the sites he excavated but to situate artefacts in relation to one another in a temporal series; the excavated site 'was merely the context for the finds' (Lucas 2001a: 31) which were recorded in three dimensions rather than in stratigraphic contexts. This preoccupation with artefact sequences to the exclusion of other features of context was not an unreflective, as yet unrefined mode of practice. Richard Bradley (1983) makes the case that Pitt Rivers's fieldwork methods were designed to empirically substantiate what was, even by the standards of the time, a quite retrograde variant of social Darwinism, a theory of cultural evolution that was intended to legitimate British class structure and counter any move that might disrupt the existing social and economic order. Pitt Rivers held that cultural change follows the same principles as biological evolution and that technological artefacts could be expected to exhibit slow stage-by-stage change over time, characterized by the 'triumph' of objects of the greatest 'utility', the cultural equivalent of biological selection for reproductive fitness: 'survival of the fittest'. Drawing on ethnographic analogy and physical anthropology, he identified the evolution of human races with these expected patterns in the development of

material culture and used this theory, which he developed in the 1860s, to interpret an ethnographic collection he had begun to assemble a decade earlier.¹ Archaeological fieldwork was a means of further documenting the evolutionary processes that had been a consuming interest of Pitt Rivers well before he undertook the excavations that made him famous. So, far from his excavations being a model of a perspectival observation and recording, Bradley describes them as ‘among the earliest attempts to bring anthropological theory into the planning of archaeological excavation’ (1983: 9); not only were they informed by Pitt Rivers’s distinctive brand of social Darwinism, but they were motivated by explicitly conservative political commitments (p. 7).² This robust conceptual scaffolding had been lost from view by a selective reading of Pitt Rivers’s notes and publications that focused on his archaeological reports to the exclusion of his theoretical writing. Wheeler (1954: 25), for example, was aware of Pitt Rivers’s theoretical and political interests in the evolution of artefacts, but discussed neither their intellectual context nor their importance for Pitt Rivers’s archaeological practice (Bradley 1983). Even though Wheeler himself explicitly rejected the view of an ‘American writer’ (Walter Taylor) that ‘the archaeologist, as archaeologist, is really nothing but a technician’ (1954: 228, quoting Taylor 1948: 43),³ he described archaeology as ‘primarily a fact-finding discipline’ (p. 228) and constructed an origin myth for professional archaeology in the UK that entrenched a separation of theory and interpretation from the production of empirical data by means of properly ‘objective’ scientific field practice.

As in the case of Pitt Rivers, but a century later, Philip Barker’s excavations on post-Roman Wroxeter and the medieval motte and bailey castle at Hen Domen have been lauded for setting a standard of meticulous excavation and recording. Wheeler-style stratigraphic excavation had dominated British archaeological practice from the 1920s, his Maiden Castle project of 1937 having established the norm of fieldwork designed to uncover and document the sequence of deposits that comprise a site. His primary legacy was a suite of standardized methods that put a priority on establishing the ‘linear “story” of a site rather than the spatial patterning of the activities carried out in successive phases of use’ (Cunliffe 1999: 376);

excavation within grid squares limited the visibility of extended features, but standing baulks preserved sections in profile that were an invaluable resource for observing and documenting site stratigraphy. Open-area excavations were adopted more frequently for sites with shallow occupation in the 1950s and then deeply stratified sites in the 1960s (see below), and Barker was an influential exponent of the practice (1969, 1977). ‘Total, or near-total, excavation’ was intended to ensure that ‘every cubic centimeter of soil is made to yield the maximum information’ (1969: 220–1), documenting horizontal contexts as well as depositional sequence. It is not hard to find statements in Barker’s publications that reinforce the perception that he embraced the ‘narrow empiricist’ horn of the dilemma we described in the previous chapter, dedicated as he was to exposing and recording whatever was to be found in the ground in as ‘objective’ a manner as possible. He is on record as an outspoken critic of ‘problem-oriented’ excavation, arguing that unless constraints of time, money or resources necessitate ‘partial’ excavation, there is no justification for destructive investigation of a site ‘designed to test specific hypotheses’; the risk of missing crucial evidence and features is insupportable (1977: 37). Elsewhere, however, Barker’s views were more nuanced. Although he insisted that ‘objectively observed evidence’ could and should be separated from its interpretation ‘so far as this is possible’ (1986: 147), he stressed the difficulty of ever realizing the ideal of ‘totally objective’ excavation:

It is impossible to dig with a completely open mind – a blank sheet on which anything that turns up is simply recorded – since every excavation begins with a purpose, a problem or a series of problems to be solved. The crucial condition is that, on the one hand, the solving of a particular problem must not lead to an inflexible strategy which misses or ignores evidence of phases, periods or structures which are unexpected or do not fit with the preconceived theories on which the excavation was based, and on the other hand, whatever evidence is found must not be bent, even unconsciously, to support these pre-conceptions. (1986: 51–2)

Barker also recognized that this ‘inevitable tendency to interpret as we go along’ (p. 147) is not altogether a liability; if excavators cultivate the kind of responsiveness to evidence he endorsed, ongoing interpretation could be expected to expose the limitations of framing questions and assumptions:

Instant interpretations obtrude constantly as the site is uncovered and these have to be turned to good advantage, so that they constantly modify the questions which prompted the excavation in the first place. (1986: 104)

For this reason, Barker was, like Wheeler, sharply critical of ‘austere’ archaeologists who insist that interpretation must be deferred until the end of an excavation. He drew inspiration on this point from scientists and philosophers of science in passages that echo the constructionist arguments developed by Kluckhohn, Brew and Ford, and Thompson in a North American context in the 1930s through to the 1950s. Citing N. R. Hanson (1967), the philosopher of science who put the term ‘theory-laden’ in circulation, he argues that observation is ‘interest-directed and context dependent’ (Barker 1977: 139) and depends on the ‘particular knowledge of the observer’; ultimately, ‘only what is observed can be recorded, and observation is not an automatic process’ (1977: 143).⁴ Given the ambiguities inherent in archaeological features, ‘doubts and uncertainties’ are an inescapable element of any process of observation and recording. And from this, Barker acknowledged, it follows that it is ‘virtually impossible to exclude all interpretation, explicit or implicit, from a description of the features, layers and their relationships – the very use of words such as post-hole, floor or hearth imply a considerable degree of interpretation’ (1977: 233). The ‘element of conjecture’ identified by M. A. Smith (1955) is clearly present in the very empirical, observational claims she had treated as the only epistemically safe ground for archaeological practice; they are themselves inferential and depend on a variety of warrants in the form of material postulates about everything from soil formation processes to artefact function and cultural norms.⁵ The question is whether these warrants and the inferences based on them are ‘untestable’, as Smith had claimed (p. 3), or are irreducibly arbitrary interpretive conventions as post-processual critics briefly claimed (Shanks and Tilley 1987: 111).

In his own practice, Barker drew on many different kinds of conceptual and technical scaffolding and actively addressed the challenge of broadening and refining the resources necessary to better ‘see’ material traces as archaeological data. For example, with respect to the sometimes ephemeral archaeological remains of timber and stone structures (1977: 112), he emphasized the importance of

cultivating a detailed knowledge of the architecture of standing buildings, their forms and construction methods, as well as the experimental understanding of natural processes that play a role in the formation of the archaeological record of timber structures, for example detailing the 'life-history of a posthole' (pp. 83–7). This led him to propose a model of how the linear accumulation of small stones and refuse defines the voids left by decayed timber sills on the ground surface, a highly specific, jointly material and cultural set of warrants for 'seeing' traces as specific types of features and constituting them as evidence relevant to broader interpretive questions. In addition, his archaeology was richly informed by the history of open-area excavations of prehistoric and medieval timber buildings in north-west Europe and Scandinavia, a quite different fieldwork tradition from that which Pitt Rivers and Wheeler had initiated, to which Barker was most directly an heir. His methodological contributions, as much as his reflective comments, bring into sharp focus not only how richly interpretive but also how much of a collective, and historically contingent, accomplishment it is to 'learn to see' archaeologically.

Learning to see timber buildings

With respect to timber-built features, Barker's technical and interpretive contributions depend heavily on innovations in open-area excavations from the late 1920s onwards on the European mainland. The living spaces of Neolithic sites and later communities had been identified as pit dwellings in the context of UK settlement archaeology since the mid-nineteenth century. This interpretation was supported by references in classical literature and by evolutionary theory of the kind Pitt Rivers endorsed; it was to be expected that such 'primitive' structures would predominate at earlier stages of human evolution (Evans 1989: 438), a view Pitt Rivers also found supported by ethnographic analogies based on travellers' reports. In the case of Pitt Rivers's excavations on Cranborne Chase, these sources suggested that timber buildings would have rested on sill beams rather than vertical posts, except for small four-post 'granaries'; his trench-style excavations served to locate such features, but the scale of horizontal

exposure was too small to delineate the surviving traces of post-built houses that might challenge these expectations. As Evans puts it, 'what was "seen" was what was anticipated'; few later prehistoric post-built structures were identified archaeologically in the UK before the late 1930s (pp. 438–9). By contrast, open-area excavations in north-west Europe and southern Scandinavia had established the presence of substantial post-built houses on later prehistoric sites in the interwar years. For example, Gudmund Hatt began larger-scale excavations on Iron Age settlements in Denmark in 1927 and developed this practice through the following decade, influencing archaeologists such as Axel Steensberg who continued in this tradition in the excavation of medieval sites through the 1940s and 1950s (Barker 1977: 16–20).

In Germany the Neolithic settlement of Köln-Lindenthal, published in 1936 (Buttler and Haberey 1936) was exemplary of this mode of larger-scale, open-area excavation of timber buildings. Impressed by the accomplishments of continental practice, especially German settlement archaeology, the Prehistoric Society decided to support the complete excavation of a later prehistoric settlement in the UK. The Society's Council chose the Iron Age site of Little Woodbury (Wiltshire) and recruited the excavation director Gerhard Bersu, who came to Britain after being dismissed by the Nazis from his job as director of the Römisch-Germanischen Kommission. Bersu's aims, field practice and interpretations at Little Woodbury 'reflected current German excavation methods of which he was probably the outstanding practitioner' (Evans 1989: 441); it was, in fact, his students who had excavated the Köln-Lindenthal settlement. In two seasons, in 1938–9, Bersu excavated about one-third of Little Woodbury using a strategy of opening a sequence of alternate, five-metre-wide strips across the site. Even though the whole excavated area was never visible at any one time and the project of a complete excavation of the settlement was curtailed by the outbreak of the Second World War, Bersu was able to identify, for the first time in the UK, the plan of an Early Iron Age farm with two large, centrally placed, timber-post round houses and a number of four-post structures interpreted as granaries, corn-drying ovens, storage pits and shallow working hollows (see [Fig. 2.1](#)). Open-area excavation allowed him to

identify post features that had not been recognized previously on Iron Age sites, and to produce a range of contextual data that trench- and grid-style excavation typically did not capture. In addition, he drew on an expanded repertoire of archaeological comparanda from continental excavations and from pre-dynastic Egypt, as well as more fine-grained ethnographic analogies that informed his attribution of functions other than habitation to various types of pit features. Although his use of ethnographic analogy has been described as ‘rule-bound’ and ‘somewhat mechanical’ (Evans 1998: 189),⁶ he was, for example, attentive to the details of the structure of storage pits with protective inner containers used by the Omaha, and he had himself observed the use of ‘hollows’ for preparing the harvest for storage in modern villages in Upper Egypt; this was the basis for positing alternative storage and activity area functions for features that had been interpreted as habitations. He also drew on ethnohistoric accounts of earth lodges in North America and construction techniques in north-west Brazil to reconstruct the structures he was identifying on Neolithic and medieval sites. Functioning as inferential warrants, these ethnographic sources as well as Bersu’s broader repertoire of archaeological comparanda put him in a position to recognize and interpret (or reinterpret) material traces as evidence that established the existence of later prehistoric, timber-post dwellings on a UK site, decisively refuting the interpretive conventions that had stabilized archaeological ‘observations’ of pit dwellings.

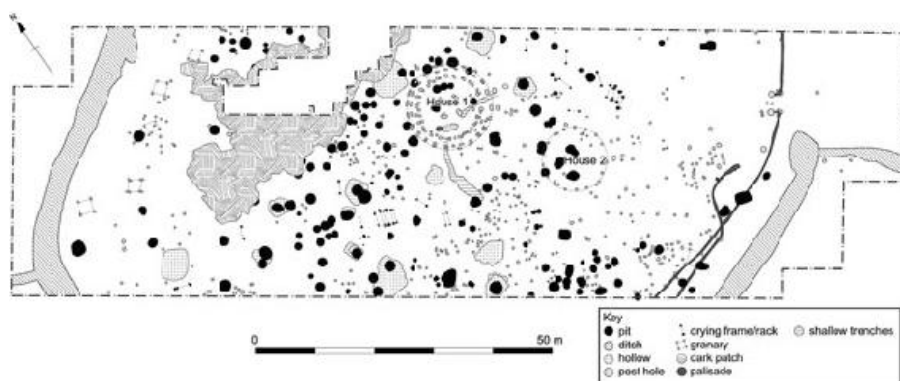


Figure 2.1 Plan of 1938–9 excavations at Little Woodbury, showing timber houses, as well as more than one phase of ‘overlapping and intersecting features’. (Source: based on plan in Bersu 1940.)

Some British archaeologists, most notably Wheeler, maintained

their belief in pit dwellings, but Bersu's large-scale excavation methods and the interpretive framework that had made timber-post structures visible quickly became the disciplinary standard in post-war UK archaeology. As timber round houses, granaries and storage pits were exposed on a growing number of Iron Age settlements, Little Woodbury came to be seen as 'an idealized settlement module' (Evans 1989: 445). A Little Woodbury settlement type, economy and culture (Wainwright 1979: viii–xi) became entrenched in UK Iron Age archaeology; for at least the next thirty years the 'slavish reiteration of the typicality of Little Woodbury' dominated archaeological thinking even though the site was only characteristic of 'a limited region of southern Britain, the chalk downs of Wessex' (Harding 1974: 21). With respect to Little Woodbury itself, this idealization obscured at least two phases of the site's occupation (palisaded and ditched enclosures), as well as a 'mass of overlapping and intersecting features' (p. 22) (Fig. 2.1), and the relationship of the site to the surrounding landscape (see Fig. 2.2): that of the larger (unexcavated) enclosure of Greater Woodbury, and of smaller enclosures and ditches revealed by aerial photography. More generally, it fostered a preoccupation with Late Iron Age round houses often at the expense of rectangular ones.

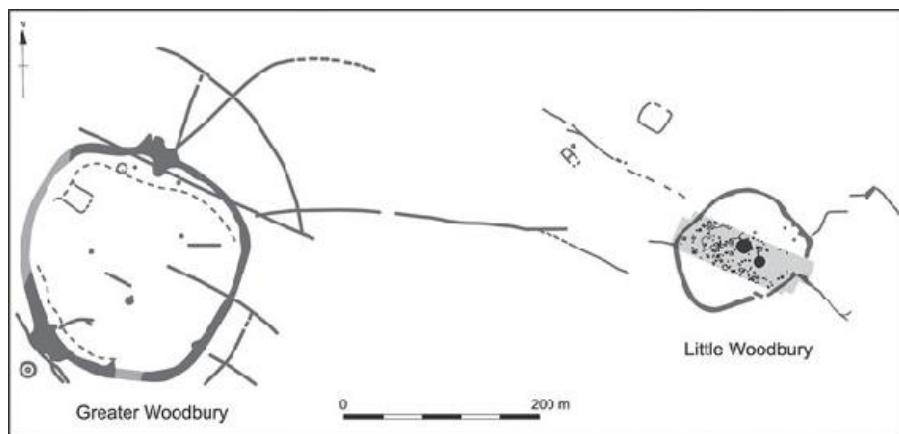


Figure 2.2 Little Woodbury in relation to Greater Woodbury and features of the surrounding landscape identified by aerial photography. (Source: based on Bersu 1940: Fig. 1.)

At the same time, British archaeologists learned to 'see' timber dwellings in a number of other historical contexts, most notably on

post-Roman sites. The stimulus here was not only the example of Danish Iron Age and German Neolithic settlement excavations that had been such a powerful influence in late prehistoric archaeology in the UK, mediated through Bersu's work at Little Woodbury, but also Bersu's own excavations on early medieval period houses on the Isle of Man and elsewhere (e.g. Bersu 1940) and a pivotal meeting of economic historians and archaeologists at Cambridge in 1948 to which the Danish medieval archaeologist Axel Steensberg was invited.⁷ The outcome was the influential excavation, from 1950, by Maurice Beresford and John Hurst on the deserted medieval village of Wharram Percy, and then, from 1953, by Brian Hope-Taylor on the first millennium AD site of Yeavinger (Hope-Taylor 1977). In these contexts, and in several later examples, painstaking experimentation with open-area excavation techniques enabled medieval archaeologists to recognize and reconstruct timber buildings.⁸ This required an extraordinary level of thought, imagination, precision and patience. The archaeological features at Yeavinger were, to use Hope-Taylor's words, 'dissected' in plans and sections as part of the process of reconstructing the structural details and construction sequence of the timber buildings (1977: 34–45). Although the continental models demonstrated the advantages of open-area excavation and the potential for identifying and recording the ephemeral traces of timber structures, in each case excavators had to devise 'ways of seeing' these structures (Bradley 1997: 63) that were attuned to the challenges of particular contexts.⁹ Bradley (1997) traces the diffusion of this emerging craft tradition through professional networks of excavators who applied what they had learned to an expanding range of UK sites including, for example, the Roman town of Verulamium (1955–61), the Anglo-Saxon palace at Cheddar (1960–2), the medieval town of Winchester (1961–71) and the defended early medieval hilltop occupation of South Cadbury (1966–71). This post-war history of excavation practice in the UK culminated in Barker's excavations at Hen Domen and on the post-Roman occupation of the town of Wroxeter in the 1970s and 1980s, where meticulous attention was given to the observation and reconstruction of timber buildings that both conformed to and challenged expectations based on known forms for their periods. Barker posited a large-scale, post-Roman occupation

at Wroxeter, an interpretation which was debated and, most recently, largely rejected (Lane 2014) on contextual, structural and comparative grounds in favour of a model of smaller-scale occupation. It was the very precision and detail of his field methods that made it possible to reassess his interpretive claims, reinforcing the principle, with which he is widely associated, that the priority in fieldwork must be to make data collection as complete and systematic as possible. Barker had taken the 'seeing' of ephemeral timber buildings to its limits and, in the context of the growth of rescue archaeology and the professionalization of field archaeology, his work at Wroxeter exemplified an approach to excavation that contributed, intentionally or not, to an increasingly institutionalized division between recording and interpretation that lasts to the present day. It was also a context in which Barker taught an entire generation in the excavation and recording of timber buildings at Hen Domen (1960–92), and Wroxeter (1966–90, initially through field schools), contributing in a formative way to the experience that shaped their careers in archaeology.

A tale of one site and two excavators

Seeing and learning to see archaeologically is, then, a richly interpretive, historically contingent and collective accomplishment. 'Seeing as' requires a prepared mind (Hanson 1958: 19), but as much as the scaffolding of technical expertise, archaeological comparanda and interpretive resources enables the recognition of material traces as archaeologically relevant evidence, and as much as the recognition of anything new or anomalous depends on the backdrop of settled expectation, there is always the risk of seeing only what you look for, and of 'not seeing' what you are unprepared for.

Bradley draws attention to this 'paradox of field archaeology' (1994: 34) – that seemingly 'objective' field observations are themselves provisional interpretive constructs – in a striking early example of archaeological fieldwork that was deliberately designed as an exercise in hypothesis testing. It is the excavation of the Neolithic henge monument of King Arthur's Round Table in Cumbria, undertaken in 1937 by R. G. Collingwood, an Oxford-based philosopher who was also an avid archaeologist, although primarily of

Roman Britain.¹⁰ Collingwood's aim was to test the implications of an extensive survey he had undertaken of the results of archaeological work on prehistoric ceremonial monuments in northern and southern Britain, on the basis of which he predicted the existence of timber buildings inside the henge enclosure which, he inferred, might have been replaced later by stone settings. Bradley remarks that this 'synthesis' was prescient; at a regional level it 'would not be wide of the mark even today' given a considerably richer archaeological record for the period (1994: 28, 34). Despite considerable historic-period remodelling, Collingwood found what seemed to be intact prehistoric deposits on the site of King Arthur's Round Table that bore out his expectations: evidence of a construction sequence of the bank and ditch that defined the perimeter of the monument, within which he identified post-holes and stone-holes in several concentric rings. These features he interpreted as evidence of a timber-post building that predated construction of the earthwork and the sockets of later monoliths.

Collingwood was unable to return for a second field season owing to ill health,¹¹ and he was succeeded as the excavator at King Arthur's Round Table by Bersu after he had finished his work at Little Woodbury in 1939. What Bersu demonstrated in that second season was that the timber and trench features Collingwood had identified did not exist (for a comparison of Collingwood's and Bersu's arguments, see [Fig. 2.3](#)). On closer examination, Collingwood's 'postholes' proved to be intrusive animal burrows, and the cremation trench feature he found associated with them was an artefact of nineteenth-century modifications to the site; only the earthwork survived scrutiny as an archaeological feature (p. 30). Bradley is quick to note that the conclusion to draw from Collingwood's 'misadventure' is not that he was an incompetent excavator (p. 34). He had done an admirable job of building archaeological scaffolding for the project, assembling and analyzing as much background knowledge about henge monuments as was available; he relied on a geological understanding of site formation processes to reconstruct the depositional sequence of the site, as did Bersu; he had observed the same features 'in the ground' as Bersu later did, and provided enough detail in his records of the 1937 excavation – both of his observations

and of his interpretive reasoning about them – to throw into relief a number of problems that warranted further fieldwork, something he did not himself have the opportunity to undertake (p. 33). What Bersu brought to the excavation was much greater experience of prehistoric post-holes and their excavation, as well as what Bradley describes as ‘a particularly subtle understanding of site formation processes’ (p. 30), including the expertise needed to recognize that the features Collingwood had identified as post-holes might be animal burrows and should be tested for evidence that could discriminate between these possibilities.¹² Collingwood was clearly a ‘victim of his own preconceptions’ (p. 33), but Bersu was certainly not operating without preconceptions even though his excavation and recording techniques were widely seen as exemplifying ‘a stance of neutrality – of objectivity’ (p. 32). None the less, Bersu’s re-examination of the primary data in light of alternative scaffolding assumptions made it possible to identify Collingwood’s error; contrary to M. A. Smith’s fears, the ‘element of conjecture’ that informed his observations was clearly testable.

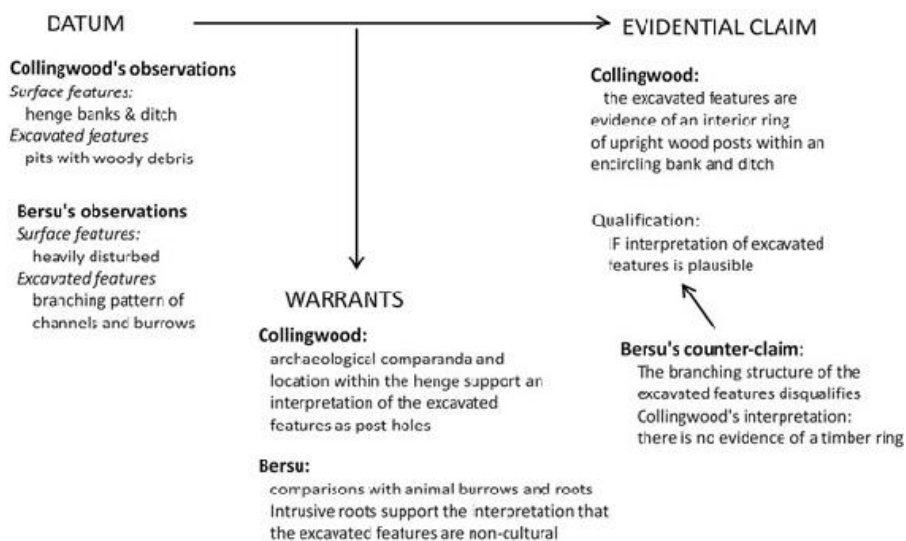


Figure 2.3 Conflicting evidential arguments: Collingwood and Bersu on excavated features at King Arthur’s Round Table, laid out according to Toulmin’s argument schema (see Fig. 1.1).

Bradley draws the lesson that ‘all techniques are imbued with theory’: decisions about where and how to excavate, what to recover and how to document it are all informed by assumptions about the

nature of the archaeological record and what it is evidence of. It is a mistake to assume that there is *an* empirical record that has the foundational status of ‘objective validity’ (p. 33) which, ironically, the atheoretical Bersu grasped and the philosophical Collingwood missed. This tale of two excavators reinforces Barker’s reflection on the uncertainties inherent in fieldwork and the primary data it produces; it illustrates his point about the importance of cultivating a stance of openness to the unexpected, responsiveness to the evidence as it emerges, a willingness to modify questions and, as Joan Gero has since put the point, to ‘honor ambiguity’ (2007). The implication, as drawn by Martin Carver, is that ‘there is no one right way of doing archaeology’; rather, there is an infinite number of right ways: what the ‘harnessed creativity’ of archaeology requires is not an aperspectival standardization of judgement, but the justification of choices that must be made as ‘appropriate’ to the goals of inquiry and to the site itself (2011: 33). In the end, however, Bersu’s reversal of Collingwood’s jointly empirical and interpretive claims was one factor that shifted the balance within British archaeology from the explicitly theory-informed ‘question and answer approach favoured by Collingwood’s generation’ (Bradley 1994: 32) to a purportedly problem- and theory-neutral mode of practice that has dominated since the 1960s. Bradley particularly regrets the tendency to privilege the documentation and recovery of material traces as an end in itself that was reinforced, in the UK, by the urgency of rescue archaeology and the growing infrastructure of conservation-oriented practice; he argues that ‘the discipline can no longer sustain the rupture between theory and practice’ (p. 34).

Entrenchment and disruption

In his influential argument for interpretation ‘at the trowel’s edge’ (1997), Ian Hodder highlights the ‘contradictions’ between Barker’s recognition that ‘recording is always an interpretation’ and the codified practices of ‘objective’ data description that were becoming established as a norm of practice in British archaeology. These include a whole apparatus of recording conventions – coding sheets and computerized forms – that rigorously separate ‘evidence from

interpretation' and 'minimize' the interpretive element (Barker 1977: 145, as quoted by Hodder 1997: 691–700). Hodder asks how it was possible that the role of interpretation came to be so systematically marginalized in these manifestly dynamic and interpretive practices of producing the primary data. Perhaps it is a legacy of positivist and empiricist traditions in archaeology, he suggests, or a response to the pressure of a growing volume of data and the need to codify these data for computer-based data management systems (1997: 2–3). When Bradley (1994) takes up this question he emphasizes the impact of the formation since the 1960s of a state infrastructure and system of funding built around goals of conservation, in which archaeological fieldwork is increasingly separated from interpretation as a matter of pragmatic necessity and of policy. Although this was a new development it had the effect of entrenching a long-standing convention in archaeological recording and reporting, rooted in the institutions of nineteenth-century scientific practice, by which fieldworkers collected the 'specimens' (including artefacts) that anchored typological analysis by 'intellectual elites' who constructed various stage-based systems of interpretation.¹³ The Danish Three Age system is an especially influential example that set the conceptual framework for prehistoric archaeology in the UK through a complex process of diffusion and contestation (Rowley-Conwy 2007, Lucas 2001a: 4–5); another is Pitt Rivers's evolutionist scheme for organizing ethnographic and archaeological material.

For those, like Wheeler (1954: 29), who admired Pitt Rivers's 'irreproachable system' of fieldwork management and recording, its great virtue was that it set a standard for dispassionate empirical reporting. Wheeler particularly emphasized his commitment to record 'every detail' observed or recovered in the course of excavation with as much precision and as little intrusion of current theoretical or personal interests as possible:

A good deal of the rash and hasty generalization of our time arises from the unreliability of the evidence on which it is based. It is next to impossible to give a continuous narrative of any archaeological investigation that is entirely free from bias. Undue stress will be laid upon facts that seem to have an important bearing upon theories that are current at the time, whilst others that might come to be considered of greater value afterwards are put in the background or not recorded, and posterity is endowed with a legacy of error

that can never be rectified. But when fullness and accuracy are made the chief subject of study this evil is in a great measure avoided. (1888: Preface, as quoted by Holmes 1889: 172)¹⁴

This ideal is strikingly consistent with norms of ‘aperspectival’, ‘mechanical’ objectivity that took shape across the sciences in the nineteenth and early twentieth centuries in what Daston (1992: 607–9) and Daston and Galison (2007: 121) describe as an ‘ethical-epistemic project’, the defining goal of which was to ‘let nature speak for itself’ (p. 120), to ‘repress the willful intervention of the artist-author, and put in its stead a set of procedures that would, as it were, move nature to the page through a strict protocol, if not automatically’ (p. 121).¹⁵ It is evident not only in Pitt Rivers’s advice for excavators, but also in the reporting conventions that traditionally divide excavation reports into separate sections: on the one hand, those that document the depositional sequence of the site (descriptions of the stratigraphic structure of deposits and features grouped into occupation phases, with sections and plans at various levels of detail) and, on the other, descriptions of the artefacts recovered (typically organized by raw material) and specialist reports (for example, dating analysis and palaeo-environmental data). Although the roots of this norm in the theoretical interests of Pitt Rivers and like-minded nineteenth-century social evolutionists will be obvious, the practice of segregating the reporting of depositional sequence from that of artefacts and other material has persisted long past the time when its originating theoretical framework had been abandoned (Bradley 2006). The result is that, as features and organic and inorganic materials are removed from their physical contexts, displaced by an archival site record that ‘stands for’ the excavated site (Lucas 2001b: 43), they come to exist ‘in decontextualized isolation’ from one another (Jones 2002: 44). This ‘fragmentation’ is further reinforced by the growing number of specialized post-excavation analyses that are commissioned for excavation reports (pp. 41–2). Samples and finds of different types are distributed to technical experts who not only work in isolation from each other and from the professional excavators who recover the material they analyse, but may have little or no contextual information about the deposits and features from which this material was excavated (e.g. Blinkhorn and

Cumberpatch 1998). Often enough they have little opportunity to take into account one another's findings and they play no direct role in assessing the evidential import of the results of their analysis. Not surprisingly, there is little acknowledgement of, much less active debate about, uncertainty in the resulting excavation reports (Hodder 1989, Jones 2002: 56); it is left to others to tease out interpretive presuppositions and possibilities after the fieldwork and reporting are complete. In practice, the prospects for deferred interpretation using these reports are often limited by the very mode of recording that is meant to produce a standardized body of data, neutral with respect to interpretive theory or investigative purpose. It is difficult to integrate the 'functional and social analyses' posited for different areas of a site given little cross-reference between the analytic reports or even between these reports and the stratigraphic and spatial coordinates for the samples and materials analysed (Jones 2002: 50–5).

Since the 1960s in the UK the division between the theory-minded, problem-oriented archaeological researcher and the data-producing fieldworker has been explicit, reinforced by professional specialization and divisions of labour that situate theoretical archaeology within universities and fieldworkers in government agencies, local government and regional contract archaeology units.¹⁶ Not only do description and data collection precede interpretation as a matter of course, but they are also increasingly practised by different people; 'an intellectual, as well as a temporal and physical, dislocation frequently separate the excavation and post-excavation stages of field projects' (Andrews, Barrett and Lewis 2000: 527). As Bradley suggests, this reflects fundamental changes in the structural and institutional contexts in which field archaeology is undertaken, an assessment echoed by Edward Harris: 'since the 1960s, archaeological excavation has dramatically changed, particularly in urban areas under pressure from new building projects' (1989: 25). Geoffrey Wainwright (2000) and Anwen Cooper (2012, 2013) make the case that the discipline was not equipped to deal with the threat to the UK archaeological resource that became apparent in the 1960s; the next thirty years witnessed a succession of attempts to address this situation by developing a national policy that created regional units and directed funding to rescue and conservation programmes.¹⁷ A pivotal development was

the 1990 legislation (known as PPG16) that established a planning process for addressing threats to archaeological sites, making developers financially responsible for any fieldwork needed to document and publish archaeological sites and material that would be destroyed by their projects. This ensured that archaeology would have 'a central role in environmental planning and regional and national policies for regeneration, tourism and land management' (Wainwright 2000: 929). The result was a fourfold annual increase in excavation from 1980 to 2010 and, given the growing emphasis on preservation of archaeological 'resources', a complementary growth in fieldwork using minimally destructive and non-intrusive methods: limited subsurface testing, survey, ground penetrating radar (Cooper 2012). At the same time, however, field archaeology was fundamentally restructured by the pressures of competitive tendering and contract excavation. The dominance of small-scale and widely dispersed projects, such as evaluations and watching briefs, has directed attention away from 'flagship sites' that might be expected to 'encapsulate the social/cultural dynamics of their periods' (Evans 2012: 304), expanding the available body of archaeological comparanda, but at the same time it has generated an enormous volume of fragmentary data that are often difficult to attribute to specific site types or features. The emphasis is on using finds to locate and date major structural features rather than pursuing other more interpretive avenues for understanding sites. The resulting 'challenge of numbers' puts archaeologists in the position of having 'to filter meaningful information from a mass of largely unusable data' (Cooper 2012: 334), providing a scant basis for advancing our understanding of prehistoric and Roman landscapes of settlements and people.

One consequence of these major shifts in the political economy of archaeology of particular relevance here is the detachment of archaeological field methods that had taken shape since the late nineteenth century from the research traditions that motivated their development. As they were redeployed in the service of culture resource management they became increasingly an autonomous technology for gathering and recording primary archaeological data, untethered to a research agenda. And with this the ambivalent separation of data collection from interpretation that had originated in

the innovative ‘objective’ research strategies of Pitt Rivers, Wheeler, Bersu and Barker came to be entrenched as an institutional, economic, even legal requirement. English Heritage instituted a commitment to ‘recording the past rather than critically analysing and interpreting it’ (Chadwick 2000: 3) as a matter of explicit policy. In these guidelines, ‘observational interpretation’ – for example, the identification of an intrusive feature as a ‘storage pit’ rather than a ‘rubbish pit’ – is considered within the purview of excavators, but ‘historical interpretation’ is to be deferred to post-excavation analysis (Andrews et al. 2000: 526–8, quoting English Heritage 1991). This deferral of interpretation is reinforced by publication practices in standard culture resource management. There can be a considerable time lag between excavation and publication, and reports on ‘outcomes’ are often relegated to a ‘grey literature’, stored in a variety of formats and locations (regional units, developers’ archives, planning authorities, historic environment records), so that regional archaeologists may not be aware of the findings of fieldwork in their region that are directly relevant to contracts they undertake. This means that the work of synthesis and interpretation becomes, of necessity, the domain of academics, and those directly responsible for producing the primary data, in the context of excavation or post-excavation analysis, are even further displaced from the process of constituting data as evidence and putting it to work in investigation of the cultural past (Andrews et al. 2000: 527). Bradley has demonstrated, in a study of site types, frequencies and distributions for later British prehistory (2007), that a systematic integration of contract-generated data can significantly reframe archaeological understanding,¹⁸ by shifting attention away from traditional areas of study, these data provide more extensive coverage and broaden the range of comparanda. But assembling the primary data and making it useable for research purposes is an uncertain, labour-intensive process, and there is considerable incentive for academic researchers to use the increasingly limited and competitive research funding available to them for projects designed to produce new data.

One area where this reification of scaffolding for data production is especially clear is in the standardization of recording methods. Refinements in methods for deciphering site stratigraphy, developed

by Wheeler in the context of grid-system excavations, required increasingly rigorous systematization as open-area excavation became more common. Wheeler's stratigraphic analysis had been guided by the sections visible in standing baulks, but even if baulks were retained in open-area excavation they would only provide insight into highly localized depositional sequences. Harris emphasizes the importance of standardized recording forms designed to 'ensure that the stratigraphic relationships of the layers and features are fully recorded, since on many complex sites most of these will not appear on sections' (1989: 25). His own 'matrix' system formalized the recording of stratigraphic detail as it was exposed in the process of excavation and was, in turn, 'a vital precursor' to the development in the 1970s of the single-context recording system by the Museum of London (Lucas 2001a: 56–8). The single-context system, another response to the challenges posed by deep, complex stratigraphies in open-area urban excavations, takes as its basis the definition of a context as 'any single action, whether it leaves a positive or negative record within a sequence' (Westman, cited in Lucas 2001a: 58), or alternatively what the Central Excavation Unit in 1977 called 'the smallest entity, other than a removable object, about which useful data may be recorded', as defined in the course of excavation. 'Single-context' units may include the products of cultural and/or natural processes, for example: walls, hearths, floors (all typically recorded as positive units); pits, post-holes and ditches (negative units); as well as erosional and other sedimentary layers. All descriptive observations of these units – their formal attributes, components, spatial coordinates – are recorded on standardized pro forma sheets (see [Fig. 2.4](#)), cross-referenced to visual records (plans, photographs) and located in stratigraphic sequence as components of a (Harris) matrix (see [Fig. 2.5](#)). This is intended to ensure not only completeness of recording but also standardization of what is reported – both content and format – between excavators and across sites. As analytically foundational, the expectation is that 'single-context' units can be reintegrated after the fact into a variety of structurally and stratigraphically meaningful composite units such as houses, tombs and activity areas; they are, in principle, problem and theory-neutral, modular building blocks for deferred synthesis and interpretation. As an essential component of

the technical apparatus that supports contract-funded fieldwork, they have the advantage of helping to ‘overcome the time constraints of on-site interpretation and the corresponding need for phase and composite plans’ (Lucas 2001a: 58).

CONTEXT RECORD

SITE CODE		SITE SUB-DIV				CONTEXT No.		P	N	C
SHAPE--PLAN		SHAPE--PROFILE		LENGTH		CATEGORY				
EDGE DEFINITION				WIDTH		PHASE				
COLOUR				DIA		CO-ORDS				
CONSISTENCE				HEIGHT/DEPTH		LEVEL Nos.				
TEXTURE										
COARSE COMPONENTS										
DESCRIPTION										
PHYSICALLY ABOVE										
PHYSICALLY BELOW										
LATER THAN										
EARLIER THAN										
EQUIVALENT TO										
SAME AS										
CUT BY										
CUTS										
COMPONENT OF										
FILLED WITH										
AGAINST										
BUTTS										
BUTTED BY										
BONDED TO										
COMPOSED OF										
ASSOC. CONTEXTS										
UNCERTAIN										
INTERPRETATION								SAMPLE Nos.		
PLAN No.										% SIEVED
SHEET No.										EXCAVATED BY
SECTION No.										DATE
SHEET No.										METHOD OF EXCAVATION
PHOTO (M)		PHOTO (C)		SUPERVISOR				CONTINUATION		
								SHEET OF SHEETS		

Figure 2.4 Example of a single-context recording sheet.

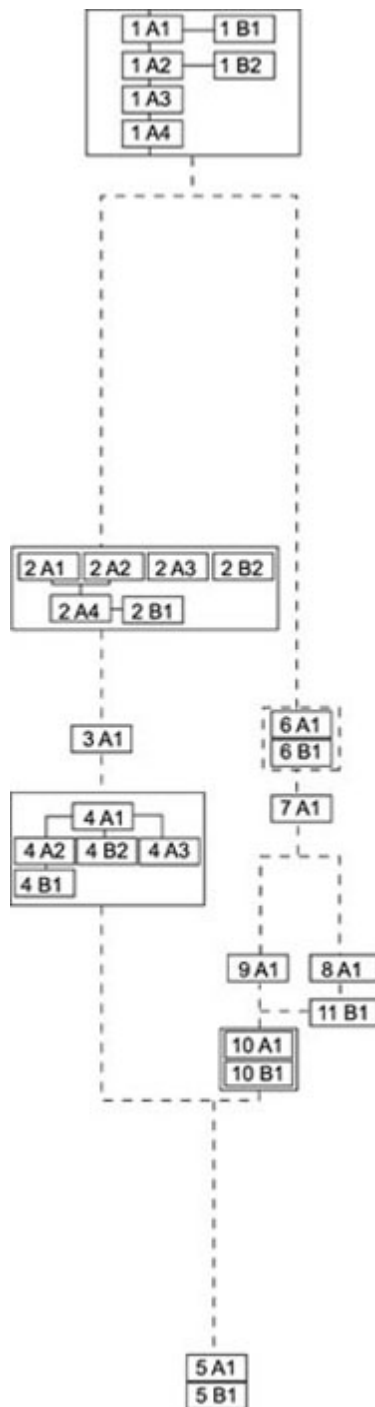


Figure 2.5 Example of a schematic matrix representing the stratigraphic sequence in trench 1 at Gatas in south-east Spain. (Source: adapted from Castro et al. 1999: Fig. 35.)

Although single-context recording is widely accepted as a best

practice among UK archaeologists, whether they work at home or abroad, a number of pointed criticisms have been raised that focus attention on the way it entrenches a problematic ideal of 'objective' field observation and recording, essentially the 'mechanical', aperspectival ideal described by historians of science (Daston 2008, Daston and Galison 2007). Methods that were 'once ... revolutionary' are increasingly 'systematized, standardized and economized' and, as they become 'the tools of a new archaeological orthodoxy', they circumscribe any opportunity for innovation and experimentation; in the process, archaeological reports become, increasingly, 'terse statements of reductionist objectivity' (Chadwick 2000: 4–5); see also Carver 2011: 44–5). Gavin Lucas describes this as a process that makes fieldwork 'more a technical than interpretive process' (2001a: 55), despite the fact that, however much field observation and recording are regimented, these practices are as deeply interpretive as ever. The question of what counts as a basic stratigraphic unit is particularly fraught. The definition of 'useful' is clearly not neutral with respect to purpose and presupposes some set of 'pre-understandings' about what it is the fieldworker is excavating and recording. In addition, questions about the interpretive rationale for treating a limited repertoire of visible depositional units and features as 'basic', long since lost from view, are reopened by the development of techniques for detecting stratigraphic units and morphological features of deposits that are invisible to the naked eye (e.g. less than 1 mm thick); single contexts become multiple contexts when granular-level laboratory analysis of samples taken from features and thin-sections of stratigraphic sequences makes it possible to distinguish depositional units that are too small to be identified as separate contexts in the field (Lucas 2001a: 154; Matthews, French, Lawrence and Cutler 1996). Further ambiguities arise with respect to the relationship of excavated materials to their contexts that are not readily accommodated by the conventions of single-context recording when, for example, it is unclear whether objects found in floors are intrusive as a function of their use or were embedded in the process of construction (e.g. Roskams 2001: 213).

These criticisms are the motivation for several recent attempts, both at home and abroad, to rethink the scaffolding of standardized

site recording systems that dominate UK archaeology (e.g. Chadwick 2003). One response is to design recording systems that preserve the integrity of complex archaeological features and structures, rather than reducing them to their discrete, constituent components. In Spain, Vicente Lull and his colleagues have argued that, in privileging what is assumed to be ‘objective’ description over historical interpretation, single-context recording undermines the potential to recognize and study, in the field, interpretively meaningful structures. Furthermore, later integration of these basic units into larger-scale elements is canalized by judgements made in the field that reduce complex observations to conventional minimal units and there is scant opportunity to revisit or reassess them inasmuch as the original deposits will have been destroyed (cf. Carver 2011: 46). The alternative proposed by Lull and his colleagues is a more explicitly interpretive, top-down approach in which the basic unit of analysis is the ‘conjunto’, roughly translated as ‘whole’ or ‘ensemble’. These are ‘historical, socio-economic units’, such as complete structures or structures at different phases of construction and use, that are explicitly constituted with reference to relevant archaeological comparanda – based on previous experience in excavating sites of the same type and period – and a range of background knowledge that informs the identification of architectural components and site-formation processes. Each conjunto is comprised of several natural and social subconjuntos (for example, roof collapse, hearths, walls and floors may be distinguished within a house structure) and of excavated materials (for example, artefacts, human remains, environmental samples). The conjunto is understood to be a hypothesis formulated in the course of excavation which makes interpretation explicit during, not after, the fieldwork. As such, it is expected that in the course of excavation it will be continuously assessed and modified, a process which ensures that excavators are actively and directly involved in the production of archaeological knowledge.¹⁹

Perhaps the best-known example of the deliberate reintegration of interpretation into fieldwork is the method of ‘reflexive excavation’ advocated by Hodder and implemented at Çatalhöyük (Hodder 1997, 2000). In recognition that ‘interpretation occurs at the trowel’s edge’

(Hodder 1997: 693), Hodder recommends a number of strategies for counteracting the routinization of 'objective' excavation methods that are designed to keep them 'fluid and flexible', ensuring that they are understood to be hypothetical and are subject to continuous testing and refinement (Hodder 2000; Farid 2015: 63–5).²⁰ The crucial insight here is that this requires a deliberate restructuring of all aspects of the work discipline of a field project; it is not just the intellectual scaffolding that requires reconfiguration but the material, technical practices and aligned divisions of labour as well. For example, so far as is possible laboratory analyses are not deferred until after the fieldwork is done but are ongoing during the field season, with the aim of ensuring that the results of the analysis of finds and samples are continuously fed back into the excavation process and that analysts have an immediate understanding of the context of the finds and samples they analyse. Fast-tracking samples for analysis makes it possible to test the working interpretation of deposits as they are being excavated, sometimes quite dramatically changing how 'single contexts' are described (Farid 2015: 65–9). New categories for recording and analysis are introduced as new questions are formulated, and information sharing is enhanced by the use of an integrated database, online excavation diaries and video technology and software innovations that make it possible to use tablets not only for field recording but also to access site archives, stratigraphic matrices and excavation records. Purpose-built digital and 3-D technologies enable the visualization of complex features such as burials while they are being excavated, and the integration of information generated by excavators and analysts into a site-wide Geographical Information System makes it possible to identify emerging hot spots and distribution patterns while fieldwork is ongoing (pp. 74–5). This 'instant and convenient availability of information has a direct influence on the archaeologist and allows better-informed decisions and interpretations' (Berggren et al. 2015: 443); it puts excavators in a position to pursue excavation and sampling strategies that are continuously responsive to what is being learned in the laboratory as well as in the field.

In principle, these innovations not only expand the range and improve the quality of the primary data captured in the field but also

‘democratis[e] the knowledge creation process’ (Berggren et al. 2015: 444). Everyone involved, from project directors, professional excavators and specialized analysts to students and short-term volunteers play a role in the process of constituting a jointly empirical and interpretive ‘archaeological record’ as well as in articulating its implications as evidence for broader hypotheses about the histories and lives of those who occupied the site. In practice, however, the aims of integration and democratization are not always realized. Not all finds can be analysed during a field season; tensions between excavators and laboratory specialists arise when, for example, microstratigraphic analysis requires the maintenance of baulks from which to draw thin sections, disrupting the conventions of open-area excavation (Farid 2015: 71). And the divisions of labour entrenched in the archaeological workforce are reproduced even in a deliberately reflexive project when the demands of fieldwork mean that professional excavators cannot participate in interpretive seminars (pp. 75–6; see also Berggren 2001: 20–1).

Some have also objected that reflexive strategies of this kind require the substantial budgets of a long-term research excavation (Hassan 1997: 1025; Chadwick 2003: 102), but the potential for applying them in a contract setting was demonstrated by the multi-period excavations undertaken in the late 1990s at Perry Oaks, where Heathrow Airport Terminal 5 is now located. A rare example in the UK of collaboration between academic and field archaeologists, participants in Framework Archaeology (a joint venture between Wessex Archaeology and Oxford Archaeology) developed an excavation methodology that integrates historical interpretation into the process of fieldwork. They shared a commitment to ‘study people rather than deposits or objects’; in particular, their aim was to understand how people inhabited past landscapes, continuously responding to and actively transforming them (Framework Archaeology 2006, 2010). This required a distinctive recording system that put the emphasis on developing an interpretive narrative as the site is excavated. So, for example, rather than attribute features of the landscape and settlements exclusively to the period of their construction, their material presence and social impact in later periods is recorded. This means that a field system that was constructed in the

Bronze Age is not necessarily treated as abandoned in the Iron Age; inasmuch as the banks and hedges of these fields remained major features of these later material and social landscapes, the recording system is designed to capture their role in the everyday lives of Iron Age inhabitants as well as that of their predecessors (Andrews et al. 2000: 529). The lessons to be drawn from the notable successes of projects such as those undertaken by Framework Archaeology and at Çatalhöyük are inspiring as tangible demonstrations of how it is possible to hold even the most entrenched conventions of practice scaffolding accountable to purpose.

Conclusions

The scaffolding required to ‘see’ material traces as archaeological data and to constitute these data as evidence is clearly not a strictly intellectual affair. High level theory, for example, abstract framework assumptions about the nature of the cultural subject, is certainly an essential component of this scaffolding. But the warrants that mediate evidential reasoning include, also, domain-specific models of particular cultures and the archaeological comparanda relevant to their investigation, as well as background knowledge bearing on specific elements of the archaeological record and the conditions that produced them that derive from an enormous array of fields, natural and cultural. In addition, the necessary scaffolding includes the technologies and institutional infrastructures, the trained skill and judgement by which orienting ‘theory’ and background knowledge is internalized, leavened by experience and translated into practice.

This internalization is a matter of professional enculturation, formal and informal. The centrality of fieldwork experience, of learning by doing, in cultivating the social bonds as well as the tacit knowledge and embodied skill that defines what it is to be an archaeologist is a recurrent theme in social histories and ethnographies of archaeology (Edgeworth 2003, 2006; Moser 2007). In particular, the role of field schools as ‘a rite of passage ... inducting students into the “culture(s)” of archaeologists’ (Joyce and Preucel 2002: 19) is pivotal both in entrenching and in disrupting fieldwork traditions. Field schools have never been about purely technical, ‘vocational’ training, even when

they are resolutely atheoretical, and often they are deliberately designed to experiment with new ‘ways of seeing’ and to disseminate approaches to fieldwork. The innovation of stratigraphic excavation in the New World associated with Nels Nelson and the formation of the first New Archaeology was significantly influenced by the training he received from the Abbé Henri Breuil and Hugo Obermaier at Castillo in Spain, a site that ‘between 1909 and 1914 seems to have served as a training excavation something like Koster or Point of Pines’ (Browman and Givens 1996: 83). The Koster and Grasshopper field schools were demonstration projects for the hypothesis-testing approach of the New Archaeology of the 1960s and 1970s.²¹ Recent examples of field schools that exemplify collaborative and reflexive modes of practice (Silliman 2008a, b, Walker and Saitta 2002)²² reinforce the point that training is an important locus of innovation in the formation of practitioners and the inculcation of field practice traditions. Crucially, these internalized norms of skilled practice are also externalized, built into and reinforced by the infrastructure of recording forms, standard equipment, modes of organizing field ‘campaigns’ and technical resources, as well as the funding and organizational structures that make it possible to ‘do archaeology’: in short, a whole disciplinary ecology of practice, as Witmore and Shanks describe it (2013).

Nothing is possible without this scaffolding, but if it is divorced from purpose and reified as an atheoretical technical practice it can be profoundly counterproductive. In particular, if it is presumed to deliver theory- and interpretation-free empirical foundations it undermines the very potential to disrupt settled assumptions about the past and expand our horizons that makes archaeology worth doing. The challenge is to balance ‘respect’ (Chang 2004: 43–4) for the scaffolding of shared goals, assumptions and norms of practice that enable the collective effort of constructing provisional empirical foundations for inquiry, against a commitment to keep this scaffolding, and the evidential claims it sustains, accountable to the goals of inquiry and to an evolving array of conceptual and empirical constraints. There is no stable, self-warranting, conjecture-free empirical foundation to be had, but it does not follow from this that what counts as evidence is entirely arbitrary: that the element of

conjecture is 'untestable' as M. A. Smith would have it (1955: 3). There is, on the one hand, the capacity of material traces to 'resist appropriation' recognized even by the most uncompromising of social constructionists. As Matt Edgeworth comments, 'in the midst of the reproduction of existing ideas, or the sculpting and shaping of evidence to fit into established ways of thinking, there is a hard materiality that refuses to be accommodated by cognitive moulds prepared for it, and which has the capacity to surprise, resist, contradict and reshape knowledge' (2012: 77). And, on the other hand, the history of 'learning to see' testifies to the potential for recognizing the limitations inherent in the scaffolding that has hitherto made it possible to identify data and mobilize evidential claims. This is a history of building, refining, calibrating and sometimes replacing scaffolding in response to the material traces that make up the archaeological record and through systematic appraisal of the warrants that mediate their interpretation as evidence – ensuring that foundations are serviceable as well as credible, fit for purpose.

Returning to the argument of the previous chapter, we suggest that the relevant contrast is not between fieldwork that produces an 'objective' foundation of empirical evidence on the one hand and practices that are merely speculative, 'constructing' an artifice in the image of projected pre-understandings on the other. There is a conception of objectivity to be glimpsed here – in Barker's account of fieldwork that respects the integrity of the emerging evidence, in the learnt capacity to recognize timber-post construction, in Bersu's reversal of Collingwood's test hypothesis, in the capacity of microstratigraphy and conjuntos to reconfigure recording conventions – that is very different from the classic 'aperspectival' ideal of a theory-neutral and value-free 'view from nowhere'. It is best characterized as a procedural ideal that recognizes the essentially constructed, inferential nature of observational claims about the archaeological record and, in giving up the quest for self-warranting empirical foundations, puts the emphasis on rigour and integrity in the construction of scaffolding that makes possible the recognition of material traces as archaeological. Nowhere is this iterative bootstrapping process more visible than in the creative use of old data

for new purposes, to which we now turn.

Notes

1

In the mid-1870s, Pitt Rivers made this collection available to what later became the British Museum of Natural History for purposes of illustrating his theory of cultural evolution and, most important to him, educating the public so they would better understand the social order that he believed was a given.

2

The role of theoretical commitments in fieldwork practice in the Americas has been a central focus of historical scholarship that traces the development of ‘New World’ archaeology. The ‘New Archaeologists’ of the First World War era that we discussed in the previous chapter were pivotal in advocating what Browman and Givens (1996) have described as a major paradigm shift by which it became possible to ‘see’ evidence of cultural change in the archaeological record of American prehistory. They identify a number of contributing factors, including not only the influence of geological expertise and Old World (especially Near Eastern) excavation techniques but also Boasian critiques of theories of social evolution. In European conceptions of ‘significant’ cultural change, characterized archaeologically by the major transitions of the Worsaae three age system and by Near Eastern chronologies, New World cultures were understood to be static, existing within an ‘undifferentiated time-plane’ (Willey and Sabloff 1974: 80); archaeologists could identify no ‘macroevolutionary changes’ of the kind that ‘the archaeological paradigm then current demanded’ (Browman and Givens 1996: 83). Stratigraphic excavation techniques could only be seen as applicable when, through different routes, Alfred Kidder and Nels Nelson in the North American Southwest, and Max Uhle in Peru, recognized their potential for detecting chronological sequences in the materially distinct cultures they were identifying (typically in terms of culture areas) in the Americas.

3

Wheeler says that he has ‘no hesitation in denouncing that extreme view as nonsense’ and goes on to argue: ‘A lepidopterist is a great deal more than a butterfly-catcher, and an archaeologist who is not more than a postsherd-catcher is unworthy of his *logos*. He is primarily a fact-finder, but his facts are the material records of human achievement’ (1954: 228).

4

In an account of ‘observation’ most fully developed in *Patterns of Discovery* (1958), Hanson argued that ‘theories and interpretation are there from the outset’, it is not a matter of ‘visual grist going into an intellectual mill’ (p. 10). It was Hanson who made the case that ‘seeing is a “theory-laden” undertaking’; ‘observation of x is shaped by prior knowledge of x ... [as well as by] the language or notation used to express what we know ... without which there would be little we could recognize as knowledge’ (p. 19).

5

We use the term ‘warrants’ in Toulmin’s sense (1958), and ‘material postulates’ in Norton’s sense (2003), as outlined in Chapter 1.

6

Evans is referring here to Bersu’s wartime excavations on the Isle of Man where he was interred as an ‘Enemy Alien’ (1998).

7

Gerrard describes this conjunction of events for medieval archaeology in the UK in these terms:

Within the space of ten days in June 1948 a series of meetings and coincidences began a chain of events which sparked post-war methodological and academic development. On 17 June Michael Postan, the economic historian, who had secured British Council funding for a visit from the Danish archaeologist Axel Steensberg, organized a seminar at Peterhouse, Cambridge ... Cambridge had hitherto been little involved in the archaeology of medieval rural settlement, but Postan was interested in seeing if archaeology could provide evidence for his theory of the expansion and contraction of settlement. (2003: 103)

Other coincidences included the identification of the earthworks at Wharram Percy as a promising site for investigation aimed at expanding the focus of medieval archaeology beyond ‘cathedrals, castles and abbeys’ which, three years later, initiated the ‘influential and enduring partnership of archaeologist and historian’ that resulted in the Wharram Percy project becoming ‘a bench-mark in the making of medieval archaeology’ (Gerrard 2003: 103–4).

8

See Gerrard (2003), although, strangely, he does not acknowledge Hope-Taylor’s contribution to these developments.

9

Bradley also notes the importance of training in the visual arts for Hope-Taylor and Barker, among others, in leading them to ‘see more in the ground than other people’ (personal communication with Chapman, 2015).

10

Collingwood had made significant contributions to the archaeology of Hadrian’s Wall and to Roman epigraphy (see Hingley 2012: 239–45).

11

Collingwood died four years later, having published an account of the ‘question and answer’ approach he advocated for historical and archaeological research in his *Autobiography* (1939; see Bradley 1994: 28, 32) and leaving notes for a number of philosophical texts that were published posthumously, including his influential treatise on the philosophy of history, *The Idea of History* (1946).

12

This is a good example of the importance of ‘indirect’ evidence, as described (in Chapter 1) in connection with Reiss’s ‘hypothetico-contextual’ model of confirmation. In so far as Collingwood’s initial observations at King Arthur’s Round Table confirmed what he expected to see if he was right about the structure and building sequence of these sites, he produced ‘direct’ evidence for his hypothesis. What he failed to do was consider alternative hypotheses that would also explain these observations, if true, and provide ‘indirect’ evidence for his hypothesis by disconfirming them.

13

These practices parallel the networks of collectors who supplied botanical and geological specimens to museums and research institutes, and they were characteristic of emerging scientific institutions in North America (the Peabody Museum and the Smithsonian Bureau of Ethnology) that vied for collections of archaeological material (Burns 2008).

14

Pitt Rivers makes similar remarks in the Preface to the first volume of his report, *Excavations in Cranborne Chase*:

Excavators, as a rule, record only those things which appear to them important at the time, but fresh problems in Archaeology and Anthropology are constantly arising, and it can hardly fail to have escaped ... notice ... that, on turning back to old accounts in search of evidence, the points which would have been most valuable have been passed over ... Every detail should be recorded ... and it ought at all times to be the chief object of an excavator to reduce his own personal

15

Daston and Galison focus primarily on the biological and physical sciences and are especially interested in the visual production of natural phenomena. Technologies of automated, mechanical recording (for example, photography) were prized because they held out the promise of ‘rigorous fidelity’ to the object in the form of images ‘free of human interpretation – *objective* images as they came to be called’ (p. 131). These were seen as guarding against not only ‘willful interventions that had come to be seen as the most dangerous aspects of subjectivity’ (p. 123), but also unintentional error arising from ‘the projection of [scientists’] own preconceptions and theories onto data and images’, the observer’s tendency to ‘theorize the seen’ (p. 135). Daston and Galison contrast aperspectival, mechanical objectivity with several other conceptions of objectivity that crystallize quite different ethical and epistemic norms of observational, scientific practice. The one thing these have in common is that they are all defined oppositionally; as an ideal they call for practices that have the capacity to preclude or to circumscribe whatever type of error is of most concern in a particular period or domain of inquiry.

16

See Berggren’s discussion of these divisions of labour within Swedish archaeology; ‘CRM archaeology and research archaeology stem from two separate traditions that have had few (or no) aims in common’ (2001: 18).

17

Although the specifics of the regulatory framework differ significantly, the shift of emphasis to ‘client-oriented’ archaeology in the context of ‘culture resource management’ has had a similar impact in a number of other settings. For example, L. Smith describes the implications of ‘mobliz[ing] archaeological knowledge ... as a “technology of government”’ in Australian contexts (2001: 97), and critical debate about the ways archaeological practice is reconfigured by Cultural Resource Management in the United States has been ongoing since the implementation of federal historic preservation and archaeological resources protection legislation in the 1970s (Paynter 1983).

18

We discuss strategies for making effective use of legacy data in Chapter 3.

19

Examples of this top-down, interpretive approach to field recording can be found from excavations in Mallorca (Gasull, Lull and Sanahuja 1984) and south-

20

There is a striking resonance here with the mid-century constructionist arguments we described in the previous chapter. For example, the main conclusion Brew draws from his arguments for recognizing that ‘systematization is necessary’ (1946: 60) but that it is always a purpose-specific construct – that there is, for example, no ‘ideal-complete classification’ to be had that captures a fundamental ‘objective reality’ and can serve all investigative purposes (pp. 48–51) – is that archaeologists need ‘more rather than fewer classifications, different classifications, always new classifications to meet new needs’ (p. 105). He particularly warns against the capacity of an entrenched ‘scheme’ to ‘produce a new type of archaeological conservatism’ (p. 61), a self-reinforcing canalization of what archaeologists observe and how they interpret it as evidence. It is crucial, he argues, to ‘maintain a fluid technique which can be changed as our needs change and our knowledge develops’ in face of the temptations of ‘dogmatism’ (p. 64); ‘we must continuously analyze not only our material but also our methods’ (p. 65).

21

Grasshopper was the successor to Point of Pines; both were field schools run by the University of Arizona. For an account of the evolution of field schools in the North American Southwest, see Chazin and Nash (2013), and of the Grasshopper field school, Reid and Whittlesey (2005).

22

For example, the University of Denver Archaeological Field School on the site of the 1914 Ludlow coal fields battle was explicitly designed to shift the emphasis of technique-focused field training from the ‘how’ to the ‘why’ of archaeology. The goal was to reconnect practice not only with orienting theory but also with its politics, ‘engaging the students with the political construction of the past outside the academic orientation of the project, showing them the importance of the past to the working class people of the area as well as to the more traditional middle-class audience for archaeology’ (Walker and Saitta 2002: 205). In a similar spirit, contributors to Silliman’s collection, *Collaborating at the Trowel’s Edge* (2008a), describe field schools that integrate interpretation ‘at the trowel’s edge’ with the specific aim of engaging students in ‘doing indigenous archaeology’ and, in that context, in actively ‘rethinking the sharing and production of knowledge’, making it a collaborative venture throughout, combining ‘rigorous, high-quality research’ with a commitment to decolonizing archaeology as a field (2008a: 2–3).

Working with Old Evidence

We have been arguing that evidential claims are themselves interpretive hypotheses. This view is by no means original; we join a great many others who, since at least the late 1930s, have made the case for a loose-knit family of anti-foundationalist views that we have described as various grades of ‘constructionism’ about archaeological evidence. In our account, evidential claims are the product of a chain of inferences that move from some factual ground to these claims by way of mediating warrants, the factual ground being claims about surviving material traces that are identified as primary data, or about the context and relationships of these traces that constitute various kinds of secondary data. The data invoked as ‘facts’ are themselves interpretive constructs; they figure as ‘data’ because of the role they play in evidential reasoning, not because they have exclusively empirical content or foundational epistemic status, as directly perceptual, theory-free, prior to interpretation, for example. The warrants that license the inference from ‘facts’ to ‘claims’ are themselves substantive claims which can be backed, in principle at least, by material postulates that establish jointly empirical and theoretical grounds for asserting that, given the facts cited and the context of use, we should accept the claims put forward about their evidential significance.¹

In short, evidential claims are the (provisional) conclusions of arguments that have all the components of Toulmin’s schema (1958) as outlined in [Chapter 1](#). They are, therefore, open to all the lines of critique that he envisions for ‘arguments in use’: contestation of the facts cited, demands for backing to support the warrants, rebuttals that call into question the applicability of the warrants to the case at hand, and qualifications of the strength of the evidential conclusions drawn. When archaeologists put old evidence to work in new ways they bring these arguments to the surface, reassessing the facts and the warrants that underpin evidential claims, trimming these claims to

the grounds invoked in their support and assessing them in light of alternative hypotheses² about what the data might mean as evidence.

We begin by describing three broad strategies for mobilizing old data that illustrate how this process of (re)negotiating evidential arguments works in archaeology: secondary retrieval, recontextualization and experimental modelling. These by no means exhaust all the ways archaeologists put old data to work, but we think they capture the rationale for a wide range of practices that are commonly used and have proven themselves capable of mobilizing transformative criticism, even of the pre-understandings and inferential scaffolding that made possible the capture of these data in the first place. They are also not exclusive of one another; in practice these strategies overlap and are used in ways that reinforce and provide a check on one another. With this overview in hand, we consider the details of a case in which it is possible to follow the trajectory of several rounds of reworking old evidence in which these strategies play a pivotal role: David Clarke's widely influential, controversial and highly generative reanalysis, in the early 1970s, of the legacy data produced by excavation of the Iron Age settlement of Glastonbury Lake Village undertaken by Arthur Bulleid and St George Gray in the late nineteenth and early twentieth centuries (Clarke 1972b; Bulleid and Gray 1911, 1917). Successive reassessments of Clarke's bold hypotheses about this settlement bring into focus the dynamics of an iterative bootstrapping process in which every aspect of his evidential claims has been subjected to critical scrutiny. Many of the lines of evidence that Clarke invoked as support for his interpretation of the site have been decisively rejected, but we take this to be a positive outcome. He made explicit the assumptions that underpinned his reanalysis of the Glastonbury data and that informed his bold conjectures, opening them to a process of critical scrutiny that illustrates what is involved when a research community realizes, in its practice, some essential elements of a procedural ideal of objectivity.

Strategies for mobilizing old data as new evidence

Secondary retrieval of primary data³

When setting out to make use of legacy data, an obvious first step is to locate the relevant field records and surviving collections, a matter of secondary retrieval of the primary data in a quite literal sense. This may seem straightforward, a matter of archaeological housekeeping, but it can be surprisingly difficult and sometimes yields results that both destabilize and significantly enrich long-entrenched evidential claims. Once assembled, secondary retrieval involves tracing evidential claims back to the primary data that are meant to ground them and assessing the integrity of these data. The primary data may also be reanalysed with the aim of appraising and sometimes reconfiguring claims about secondary facts of ‘conjunction’ (Taylor 1948: 150): temporal, spatial and formal patterns of association among the material traces that comprise the archaeological record. Finally, legacy data may be supplemented in various ways, for example by re-excavation designed to record features of context or recover discarded materials that were not originally documented or collected, and by bringing new technical resources to bear in analysing samples extracted from old data.

The vagaries of secondary retrieval have been described in vivid detail by archaeologists who work on Hopewell and Mississippian sites in the central United States.⁴ Most of these ‘eminent mound’ sites (Schroeder 2003) have been destroyed and those that remain are now under state or federal protection; little new excavation is permitted, so archaeologists are finding ways to work with legacy data of highly variable quality that were produced by fieldwork undertaken episodically since the mid-nineteenth century. In many cases the published reports are minimal and the surviving primary data prove to be frustratingly scarce and fragmented. For example, Adam King found that many of the documents that might link artefact collections to specific excavation contexts on Mississippian sites in the Etowah Valley (Georgia) had been lost and that a standard practice on Works Progress Administration (WPA) excavations of the 1930s⁵ had been to retain only unique artefacts and a few examples of common types; most of the cultural material was discarded (King 2003: 36).⁶ The extant records and collections that did survive were widely dispersed: ‘four different institutions [had] sponsored excavations at the site, so collections are housed in six locations ... [each of which] has its own

history, organizational system, and procedures for accessing collections' (pp. 33–6, 50–2). In the case of Shiloh Indian Mounds on the Tennessee River, a regionally significant Mississippian site that had been a focus of archaeological interest since the 1860s, the published record of excavations that had opened up thousands of square feet in the 1930s consisted of a four-page report (Welch, Anderson and Cornelison 2003, Welch 2006: 26). An enormous amount of work was required to reconstruct the history of research and make sense of the collections and records produced by these excavations; it took decades first to assemble scattered documents and then weeks of intensive work to 'piece together what [was] recorded and to discover what information is truly missing' (Welch 2006: 23–4, 28). Even when the original excavators did produce substantial reports it is not always straightforward to reconnect evidential claims with the source data that underpin them; the features reported in field notes were often described in impressionistic terms, without stratigraphic profiles or detailed enough locational coordinates to allow reidentification of the original excavation units in which these features or artefacts were found, much less to establish their spatial or chronological relationships (pp. 30, 35–40).

As tedious and frustrating as it often is, the labour of secondary retrieval can yield dramatic results.⁷ Source criticism has decisively undermined a number of 'facts' that had long sustained a ramifying network of evidential claims about 'eminent mounds'. For example, a staple of Moundbuilder narratives is that these prominent features on the landscape were all mortuary sites. Puzzled by the persistence of these claims in the face of well established counter-evidence, an archaeologist working at Fort Ancient in Ohio traced their origins to excavation reports of the 1890s and 1930s (Connolly 2003: 3–4, Connolly and Lepper 2004: 85–113) and discovered that, in fact, in one case the original excavator described a *lack* of skeletal material and, in another, he had speculated about the possibility that unidentifiable bone fragments, long since disappeared from the collections, might be human. Conventional pre-understandings of Moundbuilder culture persisted with a vengeance!

In a more constructive vein, the process of integrating disparate records of all the finds and features related to a given site can provide

the basis for analyses that generate new secondary data, in the form of distribution patterns and chronological associations. The data that result from this 'conjunctive' approach, as Walter Taylor had described it (1948), have the capacity to challenge not only specific evidential claims but also the framework assumptions that had informed trowel's edge identification and interpretation of the data that anchor these claims in the first place. A standard practice, now enabled by the computer power of Geographical Information Systems (GIS), is to assemble all the locational data recorded for a given site by generations of travellers, surveyors, collectors and archaeologists and integrate them into a single GIS database. Sissel Schroeder (2005) did this for the Mississippian site of Jonathan Creek in Kentucky, cross-checking the accuracy of maps and drawings of various eras against photographs, and field testing for old trenches and geological markers that would make it possible to tie features noted in archival records into coordinates on contemporary maps. She ran a number of distribution analyses which, among other things, generated maps for distinct styles of artefacts and architectural features that the original excavator in the 1930s and 1940s had identified as evidence for two culturally distinct, successive occupations at Jonathan Creek. She found that these style groups, systematically analysed, overlapped in ways that call this interpretation into question; they suggest simultaneous, perhaps seasonal occupation rather than wholesale cultural alternation (p. 65).

To build this argument, Schroeder not only scrutinizes the primary data and reanalyses the distributional patterns that were the basis for the original evidential claims, but also constructs a critical history of the theoretical commitments that informed WPA-era excavations. The assumptions that functioned as warrants for evidence that underpin the narrative about distinct ethnic-group occupations at Jonathan Creek were rooted in nineteenth-century conceptions of Indigenous cultures as highly conservative, characterized by self-contained systems of norms that were manifest in regional stylistic traditions and associated with distinct populations.⁸ Within this theoretical framework, a central objective of archaeological fieldwork was to document stylistic variability within a site and across a region that could be used to identify distinct archaeological cultures which, in

turn, could serve as evidence relevant for reconstructing histories of migration and interaction among populations bearing these cultural traits (pp. 57–9). Critiques of these assumptions had, since at least the 1950s, opened space to consider a range of more dynamic models of these ‘eminent mound’ communities. In conjunction with a much richer and more fine-grained body of data, this shift in the orienting theoretical framework disrupted the interpretive warrants in light of which stylistic variation would have seemed obvious evidence of the presence of distinct cultures.

Schroeder’s reanalysis of Jonathan Creek took her back to the field as well as into the archives and collections. In this case, as in many others, strategies of secondary retrieval involve seeking out supplementary primary data that will make the legacy data (re)useable as evidence. In addition to ground-checking the locational data captured by archival records, as at Jonathan Creek, this sometimes involves reopening earlier excavations with the aim of checking the contents of the back-fill for archaeological material discarded by earlier excavators.⁹ Although finds recovered by this kind of secondary retrieval typically lack any very specific provenance, they can provide a basis for reconstructing a more complete profile of the cultural material associated with a set of features or for the site as a whole. Sometimes re-excavation makes it possible to locate traces of recorded features in the profiles or floors of old excavations that settle specific questions about depositional history and provenance. The occupational histories of ‘eminent mound’ sites have been significantly refined in recent decades by reconstructing stratigraphic profiles for old excavation pits and trenches that make it possible to tie features with insecure provenance into site-wide chronologies. This has ramifications for how the long-term development of these cultures and their regional interactions are understood. For example, it had typically been assumed that the major Hopewell and Mississippian sites must have been occupied continuously, growing in size and density as they established themselves as regional centres and extended their influence into the hinterland until they suffered precipitous collapse and were abandoned.¹⁰ This expectation was rooted in the conventions of nineteenth-century theories of cultural evolution and defined the

questions that have animated Moundbuilder archaeology for well over a century: where do these pre-contact societies sit on this continuum of cultural evolution? Were they proto-states on the model of state formation familiar from Mesopotamia and central Mexico, in which case it is their collapse that required explanation? Or were they precocious but inherently unstable chiefdoms, in which case the puzzle is how they achieved the high level of social differentiation and hierarchy, the centralization of power and degree of craft specialization associated with the major mound centres?

This whole system of interlocking evidential and interpretive claims is destabilized by the results of source criticism and reanalysis of chronological data, which show that some of the most substantial mound sites were periodically abandoned, sometimes for as much as 100 years at a time in occupational histories of 450 years (Sullivan 2009: 183–4, 203), and that even those which were continuously occupied cycled through several periods of expansion and contraction (King 2003: 60–4, 81–3, 140–3).¹¹ At a regional level, although there is evidence of widely shared stylistic conventions (e.g. the Southern Ceremonial Complex), the temporal dynamics of site occupation undermine the assumption that these commonalities can be accounted for in terms of the influence of regional centres. In some cases seemingly dominant ceremonial centres such as Hiwassee Island proved to have been abandoned during the very periods in which their influence was assumed to have been at its height (Sullivan 2009: 8), and in others persistent anomalies suggest that regional styles were a thin ‘veneer’ overlaid on robust local traditions (McGimsey et al. 1999 (2005): 92). The process of reassessing specific evidential claims puts pressure on broad theoretical commitments; the insights generated by reanalysing the spatial and temporal distribution of stylistic traditions suggest more dynamic social interactions than acknowledged by dominant narratives about static Indigenous cultures, and the new chronologies of site occupation suggest that Mississippian and Hopewell societies may have produced ‘eminent mounds’ by means of social dynamics that did not put them on a track to develop state-like structures. And these challenges to the expectations of conventional evolutionary schemas suggest, in turn, that focal questions need to be reframed so that they take into account a much wider range of ways

in which collective effort could have been coordinated and local communities mobilized to engage in complex cultural practices that extended across regions and were sustained (even if episodically) over long periods of time (King 2003: 140–3; Cobb and King 2005: 167–92).

The application of new analytical tools, such as absolute dating, certainly played a pivotal role in this process of reappraisal, but it was only given the secondary retrieval and reanalysis of old data – establishing secure provenance for datable samples and depositional histories for the sites from which they were drawn – that radiocarbon dates could function as evidence that was pivotal in reassessing the assumptions that underpin local and region-wide interpretations. Together these conjoint strategies of secondary retrieval can be game-changing. The literal retrieval and source criticism of primary data, both records and material finds, and the reanalyses by which secondary claims about their interrelationships are appraised, can significantly reconfigure the ‘facts’ that ground evidential claims. This process of critically scrutinizing the facts can, in turn, destabilize not just the interpretive and explanatory hypotheses they had been recruited to support as evidence, but also the scaffolding of assumptions that had informed their capture and systematization in the first place.

Recontextualizing primary data

A second, overlapping family of strategies for redeploying old data is more directly aimed at reassessing (or replacing) the warrants that underpin inferences about the evidential significance of the ‘facts’ of primary and secondary data, rather than appraising these data themselves. One such strategy, especially prominent in contemporary discovery narratives about archaeology, is a matter of bringing technical resources to bear that make it possible to extract new data from old. As indicated, if provenance can be established and the risks of contamination controlled, archaeological chronologies can be significantly refined by dating samples drawn from materials collected long before radiocarbon or other physical dating techniques were widely available (Libby 1952; Marlowe 1999).¹² We discuss the

impact on evidential reasoning of the multiple radiocarbon revolutions, as well as of stable isotope and trace element analysis, in the next chapter

A related strategy turns on introducing new interpretive scaffolding in the form of theoretical frameworks and interpretive resources that were not available or not considered when legacy data were originally recovered.¹³ This plays a central role in the reappraisal of evidential claims about the mortuary features on ‘eminent mound’ sites mediated by assumptions that reflect a nineteenth-century fascination with the exotic and the savage. Mississippian ceremonial centres such as Aztalan (Wisconsin) and Cahokia (Illinois) do include mass burials, some with disarticulated and dispersed remains and some showing trauma indicative of violent death, which has been taken as evidence of cannibalism and human sacrifice. These claims have proved hard to dislodge as they resonate so strongly with popular stereotypes (Cunningham et al. 2003: 2); what is needed in such cases is not so much critical scrutiny of the primary data (the strategies of secondary retrieval just described) as a systematic reassessment of the background assumptions that function as inferential warrants. The conviction that these interments must be evidence of cannibalism is only plausible given ethnocentric assumptions about mortuary practices; disarticulated and dispersed skeletal remains are the archaeological signature for a variety of mortuary traditions that involve elaborate preparation of the dead, disarticulation and dispersed secondary burial, but are rarely associated with cannibalism (Goldstein 2006; Goldstein and Gaff 2002). In a compelling and somewhat ironic challenge to ethnohistorically impoverished assumptions about mortuary evidence, Estella Weiss-Krejci (2005) draws attention to the burial practices typical for members of European royal dynasties and aristocracies. Evidently the veneration of these elites has involved a remarkable array of body-processing practices, including evisceration, defleshing, treatment with salts and dyes, separate burial for disarticulated body parts, as well as exhumation and relocation in a series of secondary burials. Some of these practices produce just the kinds of material record that have been interpreted as evidence of the barbaric and exotic when observed in the context of North American mound sites. Questioning the

warrants that mediate these evidential claims and introducing alternatives that are consistent with the archaeological ‘facts’ constitute a rebuttal to these claims in Toulmin’s sense: they challenge the scope and strength of the initial evidential claim, in some cases eliminating entrenched interpretive assumptions as implausible and otherwise opening up new ways of understanding the data as evidence.

A third recontextualizing strategy is a matter of reinterpreting old data in the light of an expanded range of archaeological comparanda; it directly exploits the capacity of archaeological data to ‘resist appropriation’ in the terms set by an established interpretive framework. Reassessment of the spatial distribution of stylistic elements at Jonathan Creek and of the history of occupation at Hiwassee Indian Mound depended not only on reanalysing legacy data – subjecting claims about ‘conjunctives’ to source criticism – but also on resituating these data in the context of archaeological data that had been recovered since the time of the original excavations. Anna Boozer (2015) describes just such a case in which an accumulating body of primary data proved to be messy, complicated and ambiguous in ways that violate the expectations of patterned variability captured by typological categories that originate in legacy data and provide the framework in terms of which subsequent data have been described and analysed. In the course of excavating a Romano-Egyptian site (Trimithis, Roman Amheida), she encountered the remains of a house that did not fit the types of domestic architecture recognized for the region and period: it lacked a second floor and a central room with a roof, and showed evidence of food preparation and other activities in interior spaces rather than an exterior courtyard. Retracing this typology to its roots in the house forms identified at Karanis, a heavily referenced Romano-Egyptian site excavated in the 1920s and 1930s, Boozer discovered that, despite its canonical status, the documentation of this site was ‘partial and impressionistic’ (p. 104). Stratigraphic issues had never been resolved, and the relationship of domestic material to architectural spaces and features was not reported. Of particular importance where the Karanis house typology is concerned, the site report describes only the largest houses, and the artefacts recovered from these houses are characterized in terms of

material types on a site-wide basis rather than by context; there was no documentation of the architectural layout of the most common, more modest houses, or of the 'facts' anchoring attributions of function to specific rooms or activity areas (pp. 100–2). In short, source criticism of the legacy data from Karanis prompted by excavation of an anomalous house form led Boozer to critically reassess the typological conventions that had set the terms in which Roman Egyptian domestic architecture had been described and analysed for decades. Finding that this typology was based on an arbitrary selection of houses from a single, poorly understood site, she argues that it should be reconfigured to take into account the diverse forms of house compounds excavated in the late 1990s on neighbouring sites; the working typology for Roman Egyptian domestic architecture should include a 'spectrum of house options' (p. 105). While this sounds straightforward enough, Boozer reports stiff resistance to any proposal to break with the Karanis-anchored typology; the 'burden of proof' is on those urging reform to demonstrate, for example, that the anomalies they invoke are not due to poor preservation, and that the new comparanda are not marginal cases that should be set aside (p. 102).

As scaffolding that enables description and comparison, typologies are a crucial 'repository of interpretive insight' (p. 98), and an important medium of professional communication. Boozer acknowledges that, tyrannical though they may be, they are indispensable. In an analysis that resonates with objections to the quest for an 'ideal complete' typology discussed in [Chapter 2](#), Boozer argues that the 'best guesses' archaeologists rely on to bring initial order to messy archaeological data must be held accountable empirically, both to the growing body of data they are used to systematize and to evolving background knowledge (empirical and theoretical) about the nature of this material. If these are 'ill-conceived and empirically inadequate', as Boozer puts it in appraisal of the Karanis typology (p. 98), they reproduce error built into them at the outset. Substantive warrants for evidence-constituting inference are built into them that have 'palpable downstream effects' (p. 104); they determine how primary data will be described, what 'conjunctive' relationships can be identified as secondary data and

what will count as evidence based on these ‘facts’. Hard as it is to pull this framework of warrants away from the material traces that ground them, it is crucial, Boozer argues (citing Gero 2007), to ‘honor ambiguity’, to resist the tendency to accept as given even the most useful of systematizing schemes.

Making this case in a very different context in the mid-1940s, John Brew (1946) advocated what we described in [Chapter 2](#) as a constructionist approach in which he emphasized the purpose-specificity of typological systems. He was a sharp critic of the McKern (Midwestern) Taxonomic System which had entrenched ‘static units of culture’ at various scales as a framework for describing the archaeological cultures of North America. As an atemporal system, these might have been useful for ‘studying diffusion’ but, he objected, they obscure the historical dynamism of these cultures (p. 52). Elevated to the status of an ‘ideal-complete-classification’ and assumed to capture a foundational cultural reality, Brew was concerned that they had become a stultifying dogmatism (p. 61). Much as Boozer has argued with respect to the Karanis typology, Brew was intent on bringing back into focus the contingent origins and the substantive content of purpose-specific classifications such as the McKern System. It was in this connection that he insisted on typological systems being recognized as analytic ‘tools’, designed to address particular questions and predicated on ‘certain basic concepts of the nature of this material’ (p. 59). They should be held open to contestation on empirical grounds and also with respect to their usefulness as scaffolding for new research problems. ‘We must’, he urges, ‘continuously analyze not only our material but also our methods’ (p. 65). Strategies of repositioning old data in light of new comparanda and of new interpretive warrants are an indispensable means of doing exactly this.

Experimental modelling

Modelling and simulation practices in archaeology are by no means primarily a strategy for deploying old evidence, but they are a context in which the use of legacy data figures prominently. Sometimes they function as a means of recontextualizing legacy data, expanding our

theoretical horizons and suggesting novel hypotheses that make it possible to reimagine what the material traces could mean as evidence. In an analysis of evidential reasoning in palaeontology and geology, Adrian Currie challenges the myopia of 'trace centric' accounts that focus on strategies of 'gap compensation' (2014a: 83); he argues that these fail to recognize a number of other streams of evidence on which historical scientists rely, specifically various forms of indirect and surrogate evidence.¹⁴ His most provocative and intriguing suggestion is that simulations of past systems and events can quite literally 'generate new evidence' (p. 189), and that the manipulation of models of these systems can function like experimental interventions. In this account, models play not just the heuristic and explanatory role typically attributed to them, but also an evidential one; model performance data constitute an additional source of evidence relevant for assessing the implications of posited features of a past system or process that goes beyond what can be directly inferred from trace evidence. They fill gaps in the surviving repertoire of traces, making it possible to build and assess hypotheses about otherwise inaccessible aspects of a target system or process.

Modelling exercises in archaeology fall along a broad continuum ranging from those that explore a hypothetical possibility-space with no expectation that they accurately describe any particular cultural past to those that are intended to be as realistic as possible, using legacy data alongside an array of new resources to represent the dynamics and trajectory of an actual cultural system (Wylie 2016). Experimental manipulation figures all along this continuum, but it is especially prominent in the context of what Mary Morgan describes as modelling practices that are designed primarily to investigate the 'world of the model' rather than a real-world target system (2012). In a recent account of model-based archaeology, Timothy Kohler and Sander van der Leeuw recognize both functions: they describe models as constructs that support the 'joint exploration of the model and its target system' (2007: 4). A classic example of an early modelling exercise that combines these approaches is the computational model developed by Kent Flannery and Robert Reynolds of the process by which a hypothetical microband made the transition from foraging to agriculture in the Oaxaca Valley in the late Holocene (8,700 to 6,600

BCE; Flannery and Reynolds 1986). They built an assemblage of models of these evolving subsistence strategies based on archaeological data that make it possible to identify the primary sources of food exploited through the period, palaeoecological data that indicate what the availability of these resources would have been over time given climate variation, and estimates of the labour requirements and dietary return associated with a subsistence strategy based on them. To probe less accessible social aspects of the process, Flannery and Reynolds introduced subroutines to simulate intergenerational information-sharing and decision-making that would have allowed the hypothetical foragers to learn from experience as they modified their repertoire of subsistence strategies.

Running the simulation in two stages, to capture first the evolution of 'wide spectrum' foraging and then the emergence of agricultural strategies, Flannery and Richards argue that their suite of models should be assessed not only in terms of its representational adequacy – a requirement that model outcomes replicate the archaeologically documented timing and sequence of shifts in subsistence practice in the Oaxaca Valley – but also by testing its robustness. This last involved experimentally manipulating the hypothesized social learning mechanisms and estimates of climate variation to see what impact this would have on the simulation. They found that if the intergenerational learning routines were disabled, the performance of the hypothetical band peaked and then oscillated in a manner quite unlike anything suggested by the archaeological evidence, and if climatic instability was significantly greater than modelled, the band's response became conservative and the transition to agriculture stalled. The performance data generated by experimenting with the model offers a number of insights into the dynamics of the cultural system, and while these do not settle the case in favour of a particular account of the transition to agriculture in Oaxaca during the Holocene, they do lend credence to Flannery's general thesis that the explanation of this major cultural transition does not require the posit of a prime mover external to the system; incipient agriculture could have emerged as an extension of foraging practices, driven by a number of interlinked social processes and environmental conditions. This is, then, a model that mobilizes a range of datasets, including legacy data, to represent

the actual trajectory of change and to explore how it could have come about.

In other cases, archaeological models are less tightly bound to a particular target of inquiry; they are ‘how possibly’ models designed, for example, to simulate the social dynamics of a generalized type of community in response to varying social and environmental factors.¹⁵ For example, the contributors to *Model-Based Archaeology* (Kohler and van der Leeuw 2007) demonstrate what can be learned by incorporating ‘cultural algorithms’ (p. 89) into agent-based models that then function as a platform for simulating the impact of various types of social organization, kinship systems, patterns of exchange and learning process on evolving settlement systems and land-use practices. In this spectrum of modelling practice, legacy data are an essential resource for anchoring models in archaeological reality. Whether or not these simulations constitute a novel source of evidence as Currie argues, they do provide a horizon-expanding framework within which to interrogate the evidential significance of legacy data. As experimental exercises they suggest novel hypotheses, enriching inquiry in the manner recommended by the geologist T. C. Chamberlin, a nineteenth-century advocate of the ‘method of multiple working hypotheses’ (1890 (1965)), and opening up new lines of archaeological inquiry designed to assess their plausibility as representational models of particular past social, cultural systems.

We turn now to the successive reinterpretations of data recovered by the late nineteenth-century excavations at Glastonbury Lake Village as a case in which legacy data were pressed into service in an iterative process that depended on all the strategies we have described for scrutinizing evidential arguments and mobilizing old data in new ways. Although Clarke’s pivotal reinterpretation of this site is now widely regarded as an empirical failure, it is an instructive example of ambitious interpretive modelling that appeared in a pioneering volume he edited on *Models in Archaeology* (1972c). It was a field-changing stimulus for rethinking not just specific facts, warrants and evidential claims, but also the archaeological practice of reasoning with evidence itself.

Glastonbury Lake Village

Clarke's provisional model

A small Iron Age settlement discovered in the peatlands of the Somerset Levels in south-west England, Glastonbury Lake Village was almost totally excavated initially by Arthur Bulleid from 1892 to 1898 and in collaboration with St George Gray from 1904 to 1907 (Bulleid and Gray 1911, 1917).¹⁶ The preservation at Glastonbury and the neighbouring contemporary sites of Meare Villages East and West was unparalleled. The finds reported by Bulleid and Gray include structural materials such as wattle and daub walls on timber posts and traces of thatched roofing; organic and inorganic artefacts related to a range of activities including metalworking, textile manufacture and agricultural production; and plant remains that are indicative of the site's environment during the Iron Age. The site as they described it consisted of an 'amorphous agglomeration' of some ninety mounds, mainly circular with successive clay floors and hearths, which they interpreted as habitation structures (e.g. [Figs 3.1](#) and [3.2](#)), surrounded by what they thought might be a timber palisade, all of this on a foundation of tree trunks, brushwood, clay, peat and rubble ([Fig. 3.3](#)).



Figure 3.1 Glastonbury Iron Age village, floor ii, mound LXXIV, showing baked clay hearth. (Source: Bulleid and Gray 1911: Plate XXXVII, Fig. 1.)



Figure 3.2 Glastonbury Iron Age village, section of superimposed hearths of mound LXXIV. (Source: Bulleid and Gray 1911: Plate XXXVII, Fig. 2.)



Figure 3.3 Glastonbury Iron Age village, timber substructure and palisade south-west of mound V. (Source: Bulleid and Gray 1911: Plate X.)

There the matter rested for almost sixty years until the 1970s when Edgar Tratman published the first of several rounds of reinterpretation of the Glastonbury data (1970), quickly followed by Clarke's widely

influential study, '[A Provisional Model of An Iron Age Society and its Settlement System](#)' (1972b). Tratman focused on the site's phasing and dating, and on structural variation within that frame. Clarke also attended to the specifics of depositional history and arrived at an account that was, on this score, almost identical to Tratman's. However, he was not an Iron Age specialist and his aims in analysing Glastonbury were very different from those of Tratman. Clarke's central concern was with the interpretive process in archaeology. He was dismayed that, despite the 'embarrassing wealth' of structural, artefactual and environmental data generated by sites such as Glastonbury, and the newly emerging potential for computer technology to support sophisticated analyses of these data, archaeologists relied on conventional practices that 'remain[ed] scarcely more developed today than the intuitive procedures employed by the best excavators of the nineteenth century' (Clarke 1972b: 801). They rarely took a 'conjunctive' approach to analysis (Taylor 1948); their dominant concern was to record 'the particular numbers, kinds and positions of items' observed in the course of fieldwork (Clarke 1972b: 801), not to capture what we have been referring to as secondary data. As Clarke put this point; they undertook little systematic exploration of the 'mutual relationships', and the strength and covariance of the relationships, that hold between finds and features on various 'classificatory dimensions' (p. 801). This 'artificially simplified' treatment of the primary data is regrettable, Clarke argued, not just because it represents a missed opportunity but because it undermines the capacity of these data to constrain archaeological interpretation; they are, he objects, 'easily overcome' by a practice of fitting observations to a 'single preconceived interpretive model, derived ... from some historical or ethnographic analogue' (p. 801).

Reanalysis of the data published in the Glastonbury site report presented Clarke with an opportunity to 'explore the old data in a variety of new ways'; his aim was to 'transcend the level of mere descriptive records' and see what second-order data might be extracted from the 'superabundance of information' that, he was sure, must be 'hidden in the many possible dimensions of relationship' that had hitherto gone unexamined (p. 802). He was also intent on putting

these more complex relational data to work as evidence, building and testing a nested sequence of ambitious social, political and economic hypotheses about the past. He understood this to be a modelling exercise: ‘an attempt to meet the mass of observations from a selected site with a set of experimental models and the manipulative capacity of the computer’ (p. 802). It was, moreover, a ‘long-term experimental exercise’ that he saw as open-ended. Clarke was committed to making explicit the ‘framework of assumptions’ that underpin his reanalysis of the Glastonbury data so they could be re-examined in the future, and to push ‘one interim model towards the limits of its potential’ so as to ‘expose the consequences of its chain of assumptions’, again in order to ‘facilitate testing’ of these interpretations and their evidential warrants (p. 802). To this end, he advocated developing a ‘lattice’ of interlocking lines of investigation (p. 807), each of which involved the construction of models of different kinds: some were representational and others richly hypothetical; some were intended to capture the empirical structure inherent in the archaeological assemblages or, at a step removed, the cultural and material processes that played a role in the depositional and post-depositional history of the site, while others outlined in broad strokes the type of social relations, economic system and political organization that ‘could possibly’ have produced the material record that survived at the Glastonbury site.¹⁷ To build this lattice of models, Clarke relied on several strategies of secondary retrieval aimed at discerning relational patterns inherent in the primary data reported by Bulleid and Gray that they had not recognized. He also recontextualized these data, interpreting them as evidence in light of a range of background knowledge about geophysical processes, regional palaeoecology and archaeological comparanda that weren’t available to Bulleid and Gray, as well as drawing inspiration from ethnographic and historical sources they had not considered. Critics of Clarke’s provisional account, in turn, subjected each element of this modelling exercise to scrutiny, dismantling a number of his evidential claims by challenging the relational ‘facts’ on which they were based and the credibility of the background knowledge on which he relied to warrant his interpretation of them as evidence. If we take Clarke at his word, this was exactly the kind of critical scrutiny and expansive rethinking that

he hoped his 'provisional model' of Iron Age Glastonbury would provoke.

Consider, then, how Clarke built the case for his multilayered, composite account of Iron Age Glastonbury. Its centrepiece is a set of four data-intensive models of relational patterning teased out of Bulleid and Gray's data by means of secondary analyses. These were intended to capture the vertical and horizontal associations among features and artefacts, and the relationships between categories of structural features and artefacts. Clarke's secondary retrieval of 'vertical spatial relationships' from the primary data – a 'reassembled excavation plan' based on the published maps and stratigraphic relationships that had been recorded between structural features and artefacts – resulted in a temporal model of four 'crude' occupational phases, and his analyses of 'horizontal spatial relationships' were the basis for positing the existence of up to seven 'modular units' at each phase of the site's occupation (p. 815). The analysis of relationships between structures and artefacts, combined with interpretive warrants for inferring their function and social significance, underpins the distinctions Clarke drew between the various types of structure that comprise these modular units. Each unit includes structures that he interpreted as major houses, minor houses, ancillary huts, workshops, courtyards, baking huts, guard huts, annexe huts, work floors, clay patches, granaries/storehouses, stables/byres and sties/kennels (pp. 814–27) (Fig. 3.4). What Bulleid and Gray had described as an 'amorphous agglomeration' of structures was, in Clarke's analysis, an orderly system of units that were 'the social and architectural building block[s] of which the settlement is a multiple' (p. 815). This structural model of the configuration of the site was, in turn, the basis for elaborating and testing a nested series of interpretive and explanatory models that are increasingly ambitious in scale and in inferential reach. They include a model of the social organization associated with these modular units (Fig. 3.5); an economic model of the 'integrated management' of agricultural resources available in the immediate 'locational context', and of pasturage, raw material and finished goods accessible within ten-mile, twenty-mile and eighty-mile catchment zones; and a model of a settlement hierarchy and associated system of 'reciprocity and specialization' on a regional scale.

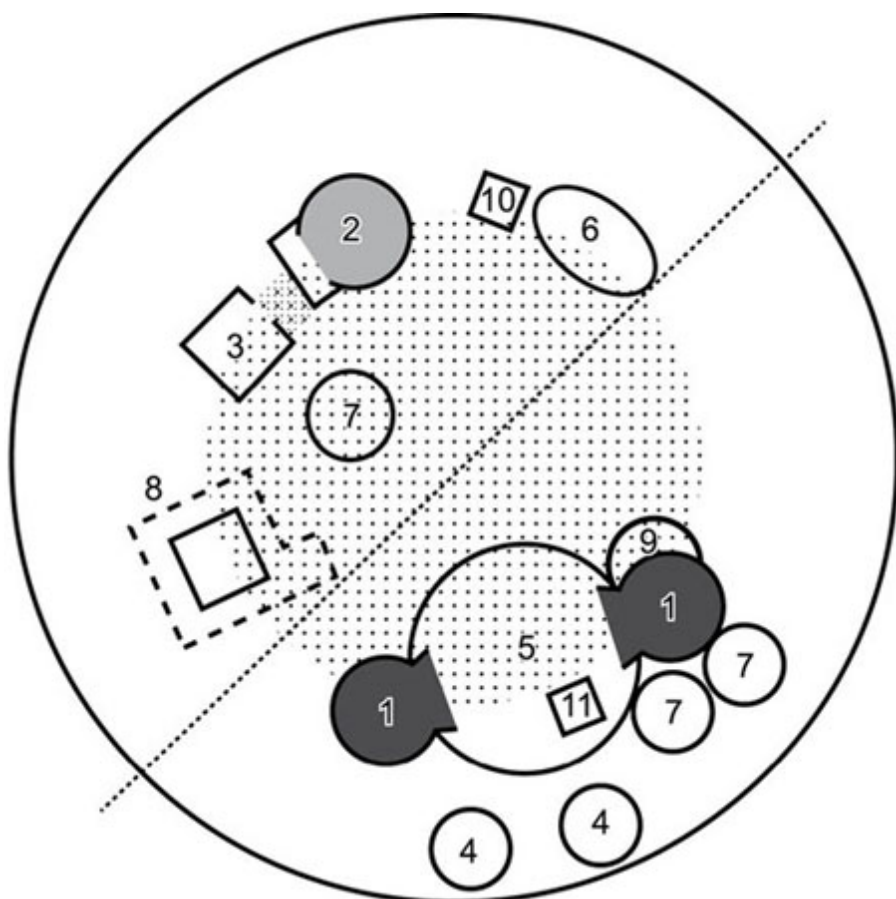


Figure 3.4 Clarke's modular unit for Glastonbury Iron Age village: 1 = Major House, 2 = Minor House, 3 = Ancillary Hut, 4 = Workshop Hut, 5 = Courtyard, 6 = Baking Hut, 7 = Workfloor, 8 = Granaries/Storehouses, 9 = Stables, 10 = Sties/Kennels, 11 = Waggon Stance. (Source: adapted from Clarke 1972b: Fig. 21.1.)

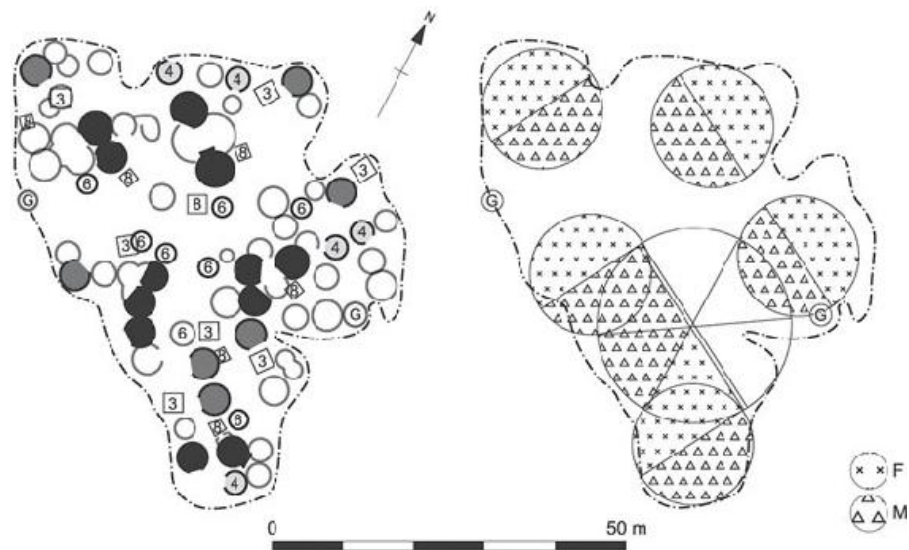


Figure 3.5 (Left) Clarke's proposed Phase IV structures for Glastonbury Iron Age village. His 'major' and 'minor' houses shaded as in Fig. 3.4, with interpretations of other structures given the same numbers as in that figure, and G = guard hut. (Source: adapted from Clarke 1972b: Fig. 21.5.) (Right) Clarke's 'tentative social model of the structural pattern' for his Phase IV of Glastonbury Iron Age village, with each modular unit divided into a 'major familial section (F)' and a 'mega-unit' reproducing the same division. (Source: adapted from Clarke 1972b: Fig 21.6)

Clarke was clear that the credibility of this assemblage of models depends on two fundamental conditions. As he put it with reference to the project as a whole, 'no archaeological study can be any better than the reliability of the observations on which it is based and the assumptions that frame the development of its analysis and interpretation' (p. 803). He acknowledged that the quality of the legacy data bequeathed him by Bulleid and Gray imposed limitations, but his point of departure was the conviction that they were 'not without pattern' (p. 805). It was worth seeing what relational patterning might be discerned through secondary retrieval and analysis; this motivating premise would be vindicated (or not) by the results of his analysis. Clarke focused instead on the second condition: explicit articulation and justification of the assumptions – the material postulates – that informed his analysis of the data and functioned as warrants for the claims he made about the evidential significance of its results. It was critical to his modelling experiment as a whole that he make these warrants precise and establish backing for the assumption that the patterning he 'extracted' by means of relational

analysis could be treated provisionally as evidence of the layout of the site and patterns of activity that took place when the site was occupied. ‘Time’s arrow’, he says, ‘left a continuous and destructive trace throughout its 2000 year trajectory’; a ‘filter of this severity’ generates a good deal of ‘background noise’ that must be taken into account (pp. 805–6). So Clarke’s first step in building his lattice of interlinked lines of inquiry was to develop a model of the post-depositional processes affecting the preservation of structural features, artefacts and refuse at Glastonbury. Although he cites specific characteristics of the geophysical context of the site in connection with his analyses of vertical and horizontal patterning, this model depends primarily on a general principle: that ‘discarded artefacts and rubbish ... [likely] suffered a “Brownian motion” of constant buffeting by animate and inanimate forces’ (p. 806). Although these forces introduce ‘noise’ into the equation, it is characteristic of Brownian motion that ‘the buffeted particle will probably remain within a small radius of its point of deposition, given a long time span’ (p. 806). Combined with the ‘repetitive redundancy’ of activities on the site over the long period of its occupation – a factor that could be expected to compensate for the loss of information – and a recognition that the material ‘signatures’ of later activities would most likely predominate in the surviving archaeological record, Clarke concluded that this post-depositional model justifies proceeding on the assumption that the relational patterning identified by means of secondary analysis reflects, at least at a composite level, the ‘original site structure and activity patterns’ of lives lived at Glastonbury when it was occupied. In the terms of our earlier analysis, this is a strategy of recontextualizing the assemblage of legacy data generated by Bulleid and Gray’s excavations. Rather than see it as ‘amorphous’, Clarke offers a model in light of which it is reasonable to treat the results of secondary analysis as evidence relevant to archaeological questions about the cultural past.

Clarke’s posit of a ‘repeated social unit’ not only is grounded in the facts of relational patterning that he identified – the recurrent association of distinct types of structure and activity area – but also depends on an appeal to regional comparanda. He reports that the modularity he attributes to Glastonbury is consistent with ‘the

abundant evidence on many other Late Bronze Age and Iron Age sites' (p. 827). This reinforces his claim that the relational facts to which he appeals are not just 'noise' and provides 'tentative' grounds for the expansive social interpretation of the site he bases on the evidence of architectural and functional modularity: that each unit was occupied by an extended patrilocal family group of fifteen to twenty individuals typically representing three generations in a village that grew from roughly sixty to 120 members over a hundred-year period;¹⁸ that these kin-linked groups were 'self-supporting and self-servicing' (p. 826); and that, given the lack of any outstandingly large or wealthy house, a broad equality prevailed among the units. Clarke did identify a central structure that may have housed a 'headman' but suggests that his role would primarily have been to 'focus community activities and coordinate the competing requirements of the family units' (pp. 835–6). He also hypothesized that the internal space of the modules was organized along gender lines, with shared functional spaces but separate and complementary living spaces for men and women (p. 827). Finally, drawing on Roman reports of Celtic society (p. 847) and on archaeological evidence that the Glastonbury community had access to distant sources of raw material and styles of artefact, Clarke suggested that kinship networks linked Glastonbury to other local villages and, beyond that, to a regional tribal confederation.

To build an 'economic model' of how this community supported itself, Clarke interpreted the evidence of plant and animal resources exploited at Glastonbury in light of 'locally critical relationships between the community's ethology, economy and environment' (p. 848). To infer these he first invoked the ecological conditions of Glastonbury's fenland location, specifying 'crude parameters' for farming and stock-breeding at Glastonbury given its fenland location. He took into consideration, for example, the challenges of seasonal flooding with waterlogging early in the growing season, and the risks of parasitic infection of livestock as well as the resources afforded by the marshy, riverine environment. He then sketched an annual cycle of agricultural and economic activity drawing on historical accounts of an infield–outfield agricultural system used locally in the medieval period (p. 856, Fig. 21.10), and positing a pattern of seasonal transhumance of sheep into the Mendip Hills that would have

provided the Glastonbury community with dry winter pasture. This last suggests a degree of economic interdependence among local communities that was reinforced by the presence at Glastonbury of raw materials that would not have been locally available. Clarke invokes this, in turn, as support for his conjecture that Glastonbury was embedded in an extensive regional network of economic interaction and specialization. He hypothesizes that, over time, this developed into a settlement hierarchy extending to 'higher-order' Iron Age sites such as Cadbury Castle, Ham Hill and Maesbury. It is here that Clarke makes use of a strategy of experimental modelling, building a 'chain' of inferences that pushes these models 'towards the limits of their potential' (p. 867). At its most ambitious terminus he draws out a number of model-based implications about social organization at a broader regional level, reaching beyond representational models of relational patterning and the social, architectural organization of Glastonbury to explore hypothetical, 'how possibly?' models. He asks, in effect, what would Iron Age life be like if the structural patterning he posits for Glastonbury, and the regional social and economic interdependence he posits, was reproduced at a regional scale? Although his interpretive models are static, they function interpretively rather like dynamic simulations, enabling him to draw out the social, organizational implications of the patterning he derives from the archaeological record and his reconstruction of the resources and constraints afforded by local ecological conditions.

Clarke's multi-scalar model-building exercise was, as he stressed throughout, 'provisional', 'tentative', 'interim' and 'idealized', the first stage in a project he was unable to complete¹⁹ that was intended to generate more questions than answers. He described his 'branching series of models' as 'crude and preliminary' (p. 867), and the exercise of pushing them to their limit as useful because it suggests 'ways in which they might be tested and the directions in which they must be refined' (p. 867). He recognized that the secondary data (the 'conjunctions') captured by his four analytic models as well as his interpretive recontextualization of these data are open to question. The follow-on projects he lists in the conclusion of 'A Provisional Model' include statistical testing designed to assess the robustness of the

relational patterning he ‘extracts’ from Bulleid and Gray’s unruly mass of primary data. This would provide a basis for assessing the ‘reliability’ of his strategies of conjunctive analysis and would, in turn, put to the test his orienting conviction that the data are ‘not without pattern’. He also called for systematic assessment of the assumptions that warrant his interpretation of this patterning as evidence of function, social organization and economic activity on successively broader scales. In this spirit he recommends additional field-testing of his models of the local ecology, the occupational history and the organization of the site as well as the regional settlement hierarchy. Finally, he suggests that his economic model might usefully be tested by building a quantitatively precise simulation and manipulating key variables with the aim of identifying ‘critical “bottlenecks” in the economy’ (p. 867) and, more generally, assessing the plausibility of his model of a seasonal round.

Responses to Clarke’s model

Clarke’s innovative reinterpretation of Glastonbury was immensely compelling for many. Even though it was understood to be ‘highly tentative’ (Cunliffe 1978: 102–3, Sharer and Ashmore 1993), and despite ongoing disagreement about many of the specifics, it is recognized as a ‘classic’ (Rahtz 1993). It is cited in archaeological textbooks as well as in national and local publications as a case study that illustrates what can be accomplished by reanalysing legacy data in the light of new theories and methods (e.g. Renfrew and Bahn 1991: 36–7; Darvill 1987: 49; Bradley 1978). Carl Axel Moberg (1981) advocated the wider use of Clarke’s theoretical approach in continental and northern Europe and applied his structural and modular model to sites in Denmark, while Ann Ellison (1981) interpreted southern English Middle Bronze Age settlements and burials in terms inspired by Clarke’s Glastonbury study, as organized around a recurring modular unit. Ian Hodder (1981: 9) cited Clarke’s thesis that these units were ‘social and architectural building blocks’ and his analysis of bilateral symmetries within and among them as an important precursor to structural approaches that were transforming the archaeological analysis of settlement space a decade after Clarke’s

‘Provisional Model’ had appeared. But apart from Moberg, the strategies of secondary analysis that Clarke particularly emphasized – his use of models of structural and material relationships as a basis for drawing inferences about Iron Age society – drew little attention until the 1990s (e.g. Parker Pearson 1996; Webley 2007), and his call for critical reanalysis of the assumptions and models on which he relied was not answered until the late 1980s.

Source criticism

The first major criticisms of Clarke’s analysis of Glastonbury appeared in the mid-1980s. In a brief but scathing critique that appeared initially in French in 1982, Paul Courbin (1988) identified Clarke as an exemplar of the New Archaeology as it was taking shape in the UK, and cited his provisional model of Glastonbury as a prime example of all that he found misguided, overstated and ‘naïve’ (p. 44) in the attempts of New Archaeologists to institute a hypothetico-deductive testing methodology. Although Clarke did not claim to be testing hypotheses in this narrowly specified way, Courbin found much to object to in what he describes as a lack of ‘validation’ for the interpretive claims Clarke put forward on the basis of his reanalysis of the data reported by Bulleid and Gray. He did acknowledge Clarke’s proposal of a modular unit as an intriguing ‘structured working hypothesis’ – ‘I do not claim that all of this is wrong or without interest’ (p. 32) – but, citing Bulleid and Gray’s report, he raised a number of questions about the plausibility of Clarke’s ‘assertions’ (p. 30) about the relational patterning he had identified in their data, the spatial organization and phasing of the site, and especially his reported ‘impression’ (p. 31) of the relative wealth, the generational structure and the ancestral and kinship relations associated with his modular units (pp. 31–3). While uncompromising in his condemnation of the inferential leaps by which Clarke built his lattice of provisional models, Courbin does not himself undertake a close evaluation of the details; he was intent on pillorying what he saw as the overinflated ambitions of a new generation of archaeologists who did not recognize the wisdom of the established methodologies of ‘traditional’ archaeology (p. 36).

This task was taken up by John Barrett in a study published in 1987 that focused on the empirical bases for Clarke's broader claims rather than directly challenging Clarke's provisional model and its theoretical bases. Barrett was concerned that Clarke had 'accepted ... at face value' the data he found in the original excavation reports (1987: 414, cf. Courbin 1988: 115) and had not taken into account the extent to which the information recorded by Bulleid and Gray was itself an interpretation. In effect, Barrett objected that Clarke had skipped a crucial step in the secondary retrieval of the legacy data on which he based his study, with serious implications for his post-depositional model of the site and, by extension, his interpretive model of a settlement pattern and social organization based on a recurrent 'modular unit'.

Barrett's source criticism turns on a systematic appraisal of 'the internal logic of [Bulleid and Gray's excavation] report', its assumptions, observations and interpretations (1987: 410). He scrutinized their excavation and recording techniques, noting that they reported only a subset of the flint artefacts and pottery they recovered, and determined that when they did report the location of finds the level of detail was highly variable: many artefacts were recorded by association with particular mounds but not stratigraphically within those mounds. He also found that the rates of recovery of artefacts and other finds decreased with their distance from the clay floor areas, a trend that seemed most likely a function of the 'churned up', waterlogged ground conditions created by the 'trample' of excavators evident in published photographs (p. 413). Finally, and most consequentially, Barrett noted that, incomplete though they were, the details Bulleid and Gray did include in their published plans and sections were inconsistent with Clarke's interpretation of upstanding structures. For example, some of the features Clarke identified as evidence of timber 'walls', 'doors' and 'porches' had been found buried beneath the latest of the clay floors (p. 418). The published record of the 'palisade' encircling the site and the lines of wattle and stakes that Clarke had interpreted as walls was similarly problematic; Bulleid and Gray report instances of clay floors that overlap the tops of the 'palisade' posts and 'walls' (e.g. [Fig. 3.6](#)). Given the details he retrieves from the original site report, Barrett

proposes an alternative interpretation of these features: that they might better be interpreted as elements of a stabilizing structure that held in place the timber substructure on which the clay floors were built. In Toulmin's terms, Barrett suggests that Clarke's evidential claims must be significantly 'qualified' (in scope and in strength), both because the facts he invokes are problematic and because he has not eliminated alternative construals of these facts as evidence that would undermine his interpretive models.

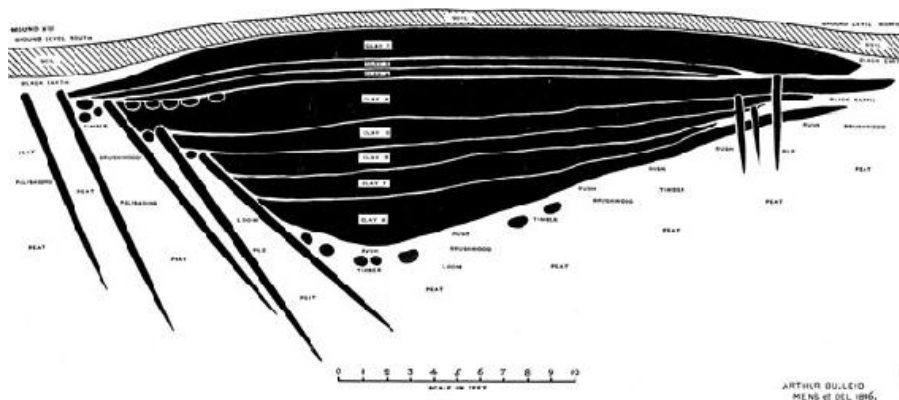


Figure 3.6 Glastonbury Iron Age village, North-South section through the middle of mound XIII. (Source: Bulleid and Gray Plate XIX top.)

In addition to this sharply pointed source criticism, Barrett also recontextualizes the major features reported for Glastonbury, the 'dwelling mounds', drawing on the results of new excavations at Meare East and West and an appreciation of the dynamic geology of fenland sites more generally. This amounts to a 'rebuttal', to invoke Toulmin's terminology again, in which Barrett directly challenges Clarke's post-depositional model, the warranting assumption that underpins all of his evidential claims about spatial patterning. The comparison with the Meare sites and palaeoenvironmental sources suggest that Clarke had overlooked an especially consequential set of post-depositional processes affecting Glastonbury: the recurrent waterlogging and desiccation of the peat matrix that would have significantly disturbed the stratigraphic contexts of the artefacts and other finds reported by Bulleid and Gray. Indeed, Barrett argues, these processes could have been responsible for the features they described as 'dwelling mounds'. He suggests that rather than evidence of long-

term permanent occupation, the Glastonbury structures, like those at Meare, might well have been seasonal; the wetter local conditions as well as these post-depositional processes could account for their more substantial clay, timber and rubble foundations (1987: 421).

The pivotal argument here is that Clarke's appeal to an abstract model of the processes that affect archaeological sites in general – Brownian motion – may not apply to Glastonbury, or at least it applies only in a highly qualified sense. To provide inferential warrant for his claims about relational patterning in the Glastonbury data, this model would have to take into account the geophysical dynamics specific to fenland sites and the evidence of variable preservation that the original excavators had reported for Glastonbury.

Clarke's 'misreading and misunderstanding'

A more extensive critique was published by John Coles and Stephen Minnitt almost a decade later (1995). But in contrast to Barrett, Coles and Minnitt took aim directly at Clarke's 'provisional model'; they intended to 'eliminate [it] for good' even though they praised Clarke for his timely, 'provocative' and 'stimulating' theoretical approach (pp. 181–2, 190). Coles had been engaged in fieldwork in the region of Glastonbury – the coastal plain and wetland area of the Somerset Levels – since 1962; he had undertaken the re-excavation of the Meare sites from 1978 and, on a more limited scale, of Glastonbury in 1984. In an earlier joint publication he had raised a number of questions about Clarke's interpretation of the structures he identified at Glastonbury (Coles and Coles 1986: 164–71) and doubts about his model of the site's environmental setting, basing these last on new palaeoenvironmental analyses published a few years earlier (Orme, Coles and Silvester 1983: 72). In the early 1990s, Coles undertook a close analysis not only of Bulleid and Gray's published report but also of the Glastonbury finds and excavation archives. Much of this material was inaccessible when Clarke reanalysed the published report, and it made possible new secondary analyses as well as source criticism that reinforced and extended many of the concerns raised by Barrett. Coles and Minnitt confirmed Barrett's finding that the pottery was not well provenanced or recorded, and they determined that not

only had much of it been discarded (especially undecorated sherds) but that most of what had been retained had never been processed. They found that sections had been recorded for only about a third of the mounds and that, even though the field notes indicated an improvement in stratigraphic recording over successive field seasons, it was still difficult to tie many of the finds (especially flint and pottery) to specific mounds or floor levels within mounds. None the less, Coles and Minnitt assembled for the first time a detailed catalogue of all the mounds recorded for Glastonbury with as much information about their stratigraphy and associated small finds as they could glean from the archival records and collections.

This robust secondary retrieval of Bulleid and Gray's primary data, and discerning appraisal of its 'reliability' (Clarke 1972b: 803), put Coles and Minnitt in a position to specify more closely the range of ways in which, in their account, Clarke had 'misread and misunderstood' the published excavation report (1995: 190). They recontextualized these primary data in terms of more robust background knowledge about the geology and ecology of fenland sites, deepening Barrett's critique of Clarke's post-depositional model in several respects. And, drawing on chronological and contextual information that was not available to Clarke or that he had overlooked, they reassessed his relational analyses, questioning his interpretation of structures and activity areas and his claims about their co-occurrence as components of modular units. The result is a critique that calls into question the evidential reasoning that underpins Clarke's models of the occupational history and organization of the site which, in turn, anchors his experimental models of the economy, and the social and political organization of Iron Age Glastonbury on a local and regional scale.

The crucial first step in this critical engagement with Clarke's 'provisional model' was to extend Barrett's (1987) analysis of his post-depositional model: the assumption that despite 'constant buffeting', the principles of Brownian motion suggest that the patterning discernible in the vertical and horizontal distribution of the excavated material approximates its original deposition. Based on their experience of excavating wetland sites and a more robust understanding of the geomorphology of these sites, Coles and Minnitt

drew out the implications of Barrett's observation that Glastonbury would have been subject to recurrent waterlogging and desiccation. In particular, they noted that at Meare the resulting expansion and contraction had produced vertical cracks in the peat that were made wider and deeper by the activity of earthworms and moles (Orme et al. 1981: 23). Although no finds had been recovered from these cracks, refitting potsherds indicates that there had been localized horizontal post-depositional movement as well as 'considerable' vertical movement of small finds (Orme et al. 1983: 71–3). Given these wetland conditions, Orme et al. had argued that material and debris would have sunk or been trodden into the soft ground surfaces of sites such as Meare and then moved further from their depositional contexts as the site cycled between flooded and dry conditions. Even so, Coles and Coles (1986: 56) acknowledged a tension between these observations and evidence at Meare of 'basic stratigraphic stability' in the form of consistent patterns of vertical deposition; for example, they found that jars consistently occur in lower levels while bowls are predominantly found in the upper levels. Taking this into account they qualify their model, recognizing that the vertical displacement of cultural material by waterlogging and mole activity was also constrained by the sequence of clay floors built on top of the peat.

Combined with the results of their archival analysis of Bulleid and Gray's primary records, this more detailed background knowledge of the geomorphology of wetland sites, especially the comparison with Meare, makes it clear that the post-depositional processes operating on Glastonbury cannot be modelled in terms of a single, general process as Clarke had proposed; they must be assessed in relation to local conditions. The more closely specified depositional and post-depositional model proposed by Coles and Minnitt also supports Barrett's suggestion that the mounds excavated by Bulleid and Gray could have been created by post-depositional cracking and compression acting differentially on superimposed floor levels of the structures and the finds contained in them, as compared with the non-structural areas (Orme et al. 1981: 23). Coles and Minnitt conclude that Clarke's analysis of the legacy data from Glastonbury and his interpretation of it as evidence is flawed because he 'did not comprehend what an archaeological excavation involved, particularly

one that was on a wet site' (1995: 182).

A further implication that Coles and Minnitt draw from their reassessment of the geological and ecological setting of Glastonbury is that Clarke misunderstood the local conditions that would have shaped patterns of land use. He had failed to consider the results of pollen analyses of the fen wetlands that had been undertaken by Cambridge botanist Harry Godwin (1955; see also Aalbersberg and Brown 2011) and did not take into account the implications of Glastonbury's immediate context at the head of an estuary on an alder–willow–fen carr. Located in what was essentially a swamp with variable water depth, it was truly a wetland site, not a settlement built on a raised bog (see [Fig. 3.7](#)). The alluvial soils and river floodplain that, in Clarke's model, would have supported cultivation and livestock grazing within the immediate area of the site did not exist in the Iron Age, so it follows that Clarke's appeal to local medieval land use as a direct historic analogy for the seasonal round at Iron Age Glastonbury is untenable (Coles and Minnitt 1995: 188–9).²⁰ This is a line of critique that directly challenges Clarke's experimental economic model; rather than calling into question the inferential warrants that mediate his interpretation of the archaeological data as evidence, it throws doubt on the credibility of the assumptions that informed his extrapolations beyond the evidence.

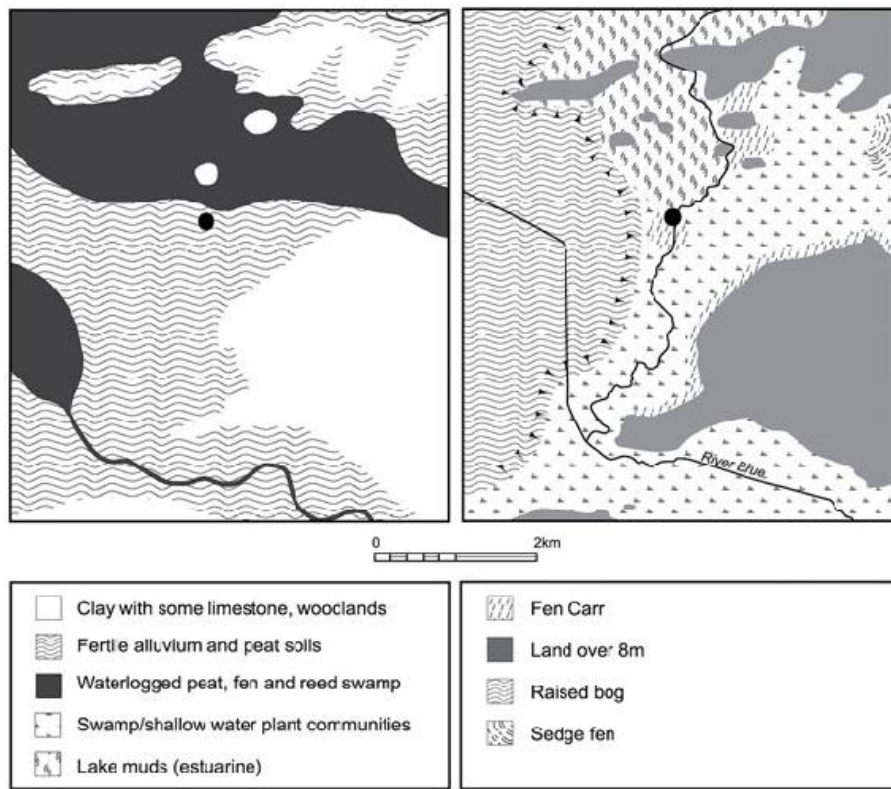


Figure 3.7 Comparison of (left) Clarke's 'speculative' environmental reconstruction of the Glastonbury Iron Age village territory and (right) Aalbersberg and Brown's most recent palaeoenvironmental reconstruction. (Sources: adapted from Clarke 1972b: Fig. 21.7, and Aalbersberg and Brown 2011: Fig. 1b.)

Coles and Minnitt also questioned Clarke's four-phase model of the occupational history of Glastonbury. Like Barrett, they noted a number of problems with the site stratigraphy as reported by Bulleid and Gray and drew the implication that, because Clarke had given no clear indication of which floors within mounds he attributed to specific phases, his phasing for the site as a whole was problematic. Using the stratigraphic data they had assembled from the archival as well as the published report, they reanalysed the relationships between individual structures (pp. 110–15); the result was a four-phase sequence, but one that differed from Clarke's. Evaluating new radiocarbon dates, including those from other local sites, in relation to 'typological dating' of pottery, metal and glass objects from the wider region of southern England, they concluded that the site had been occupied nearly twice as long as Clarke had suggested (c. 250–50 BCE).

Finally, and perhaps most consequentially, Coles and Minnitt reassessed Clarke's claims about relational patterns – horizontal, artefactual and structural conjunctives – that constitute the foundational evidence for his claim that the basis for the organization of the site as a whole, through time, was a modular social, architectural unit. Tracing his relational models back to the primary data he cites, they found that his distinctions between types of structure and his claims about their co-occurrence as components of a module are empirically unsustainable. For example, there is much less consistency in the structural attributes of the 'major' houses and much greater variation in their artefact associations than claimed by Clarke; indeed, on re-examination, some of them proved not to exist (pp. 183–7). In several cases the minor houses did not exist at the same time as the major houses with which they were associated in Clarke's account. Only one of the seven 'baking huts' Clarke had identified fits his definition of this type of structure, and the 'guard huts' lacked the structural and artefactual features he had attributed to them. There was also no evidence of 'repeated burial of butchered horse skeletons in and around' the structures he identified as 'stables/byres'. While Coles and Minnitt raised few problems with the 'clay patches' and the 'granaries/storehouses' of Clarke's model, they argued that the mound central to the site that he had identified as sufficiently wealthy to justify the inference that it housed a 'headman' was most likely not a house (Mound 42), and they pointed out that there was no concentration in this central area of material indicative of wealth such as decorated pottery, ornaments and weaving combs. This was an issue that Courbin had raised in particularly sharp terms, observing that the only basis for Clarke's 'untested' headman's house hypothesis was a series of 'assertions' about 'a mound which, while bigger than the others, seems to have been little frequented; the pottery is no different from that of the other houses, and the only notable particularity is that of less than ten weaving combs' (cf. criticisms by Courbin 1988: 30).²¹ Coles and Minnitt note, as well, Clarke's 'seemingly uncritical use' of historical sources to interpret the relational patterns he identified but do not pursue this point.²² Nor do they critically assess the model of regional settlement that Clarke proposed as an extension of the modular structure he attributes to

Glastonbury, although there were a number of alternative models of Iron Age society, economy and political structure under discussion by the mid-1990s. Their rejection of Clarke's 'provisional model' turns on the point that, if his striking claims about the modular organization of Glastonbury are unsustainable, then the plausibility of his more ambitious experimental models of a regional settlement system and social network are undermined. Overall, they conclude:

[Clarke's] identifications of structural elements on the site are based on very poor factual foundations, and misleadingly so in some cases. His proposed activity areas, wealth distributions, economic base and environmental setting are all incomplete or downright wrong. There are too many uncertainties, too many misreadings of the evidence, too much neglect of Bulleid's clear statements, too much forcing of the data into the mould chosen by Clarke to explain the village. (1995: 190)

An iterative bootstrapping process?

Coles and Minnitt, and Barrett, demonstrate that an appeal to the fluid dynamics of Brownian motion is pitched at too high a level of abstraction to serve archaeological purposes as a model of the post-depositional processes operating on a fenland site such as Glastonbury. Their source criticism and the reanalysis that Coles and Minnitt undertook of Bulleid and Gray's primary data establishes that, in many specifics, Clarke's models of the conjunctions holding among the artefacts, features and structures they reported are empirically flawed, so that his phasing is problematic²³ and his modular units do not exist in their full form across the site and throughout its history of occupation. Coles and Minnitt also make a compelling case that his economic models of land-use and subsistence practices are untenable, grounded as they are in an inadequate understanding of the local and regional ecology. In short, Coles and Minnitt succeed in 'eliminat[ing]' Clarke's provisional model as a representationally accurate reconstruction of the social and architectural organization of Iron Age Glastonbury. At the same time, however, there are many respects in which they draw inspiration from and, in fact, refine and extend Clarke's models (see [Fig. 3.8](#)).

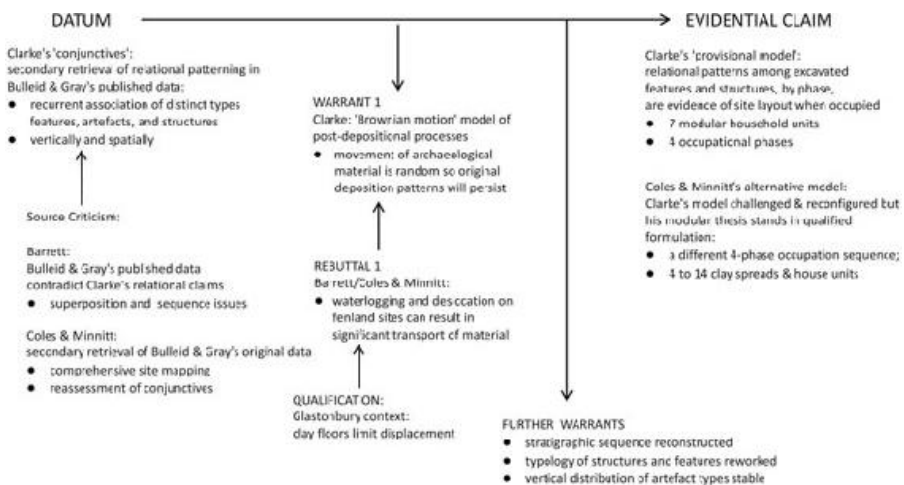


Figure 3.8 The search for non-random patterning in Bulleid and Gray's 'amorphous agglomeration' of features at Glastonbury, laid out according to Toulmin's argument schema (see Fig. 1.1).

For example, although Coles and Minnitt reject Clarke's post-depositional model and emphasize the uncertainties inherent in the original excavation data, by no means do they endorse Bulleid and Gray's verdict that the Glastonbury settlement was an 'amorphous agglomeration' of features and structures. They identify a great deal of non-random patterning in the surviving features, architecture and the artefact assemblages reported for Glastonbury on which they base an alternative model of 'village life' at Glastonbury. Drawing on their understanding of the Meare sites, they recognize principles constraining movement both horizontally (for example, structures) and vertically (clay floors as opposed to open, wetter spaces) that apply to Glastonbury. Given this more closely specified post-depositional model, Coles and Minnitt are confident enough in the locational data reported by Bulleid and Gray to use their original record of findspots to construct phase plans of the site, as well as non-phased distribution maps of categories of artefacts and features, on which basis they specify the locations of various productive activities for each of the four periods they distinguish in the history of the site's occupation. They also infer from the distribution and frequency of the activity areas and structures associated with each phase a number of distinct 'units' consisting of houses and clay spreads which make up a robust 'site structure' (1995: 198–206), as well as the existence of differences in wealth and social position among these units (p. 205).

They attribute four such units to their Early Phase, 'each with one house, adjacent spreads of clay for more open-air activities, and various storehouses of racks' (p. 198), and they estimate the population at fifty residents over one generation. The number of units increases to ten in the Middle Phase, which they estimate represents 125 people over two generations; they hypothesize, in this connection, 'an influx of families ... joining their previous relations (blood or acquaintance or work)' (p. 199). With a further increase in structurally occupied space in the Late Phase, the number of units rises to fourteen, which Coles and Minnitt estimate housed up to 200 residents over three generations. They describe these units as 'self-sustaining ... each consisting of a house with attendant yard, some with large shelters or fenced spreads attached' (p. 202). Given the scale of the structural changes they attribute to the Late Phase, they infer the emergence of 'some kind of control and direction' (p. 203) which they identify with a house in Mound 9 that has a distinct structure and assemblage of artefacts (p. 204). In the Final Phase there was a contraction in the activities and the settled area with only five houses constructed and an estimated population of no more than fifty over one generation. The upshot is that Coles and Minnitt do not unequivocally reject Clarke's broader thesis of modularity, and their own alternative model is tacitly structural; they identify an assemblage of architectural 'units' recurring throughout the history of occupation, which they interpret as 'self-sustaining' in the Late Phase and as embedded in an extended regional social and kinship network by the Middle Phase.

Clarke's models were flawed in many respects, in some cases unnecessarily given the information available at the time, but they were intended to be a point of departure for further work. As Clarke makes clear, he saw them as scaffolding in several senses: methodologically, as an exploration of the relational structure inherent in archaeological data, even legacy data, that could be captured by means of secondary analysis; conceptually, as bold conjectures that, he hoped, would inspire more expansive thinking about Iron Age society; and procedurally, as a catalyst for an ongoing process of critical scrutiny and refinement of the specifics of his models of Glastonbury and its settlement system. Viewed in this light,

Clarke succeeded on all fronts. He influenced a wide community of archaeologists, challenging their ways of ‘doing’ archaeology, introducing new methods of analysis, and recontextualizing old evidence in terms of ambitious new interpretations that provoked just the kind of probing reanalysis he had hoped for – of his claims about the empirical data, the assumptions that informed his secondary analyses and his array of working models. Taken as a whole, we see this as an instructive example of iterative bootstrapping: building on empirical and conceptual foundations that are known to be tenuous, and rebuilding them as inquiry proceeds. This is a process that, as Chang describes it, requires ‘a great deal of innovative thinking, painstaking experiments, bold conjectures, and serious controversies, which may in fact never have been resolved quite satisfactorily’, as well as ‘the rehabilitation of discarded or forgotten knowledge’, all on an intergenerational scale (2004: 4–5). The central virtue of Clarke’s model-building methodology is that it made explicit the scaffolding of assumptions on which his evidential claims depended, productively opening them to critical assessment – rebuttal, qualification, reconfiguration, extension and replacement – with implications well beyond the appraisal of his interpretive models of Glastonbury. Among other things, Clarke’s experimental approach illustrates how productive it can be to foster an ‘element of conjecture’ and demonstrates, contra M. A. Smith (1955), that the conjectures that inform the interpretation of data as evidence are by no means ‘untestable’.

Conclusions

One lesson we draw from this case, and from strategies for using old evidence more generally, is that it takes a community to mobilize the critical scrutiny necessary to hold evidential claims accountable: to assess the ‘facts’ and the ‘material postulates’ that warrant their interpretation as evidence, and the inferential moves based on this evidence. To be more specific, it takes a community that can bring diverse perspectives and types of expertise to bear, and that has in place mechanisms for critical engagement and the will to enact them. These are the fundamental requirements of the procedural conception

of objectivity that we saw emerging in critical debate about fieldwork. Another condition for successful bootstrapping is that, as we argued in connection with our analysis of fieldwork, enacting these strategies for making effective use of legacy data is not just an intellectual process. It depends not only on holding the conceptual and technical elements of scaffolding for evidence open to critical appraisal but also on interrogating the ecologies of practice – the disciplinary culture, the training and funding systems, the institutional infrastructures – that sustain (or confound) these practices. The Glastonbury case makes it clear that a crucial condition for success in this is the capacity to bring relevant expertise to bear on questions that exceed the resources of any one discipline. Clarke drew on an enormous number of fields to build his assemblage of models; that he lacked sufficient expertise to do this convincingly in all of them should not be surprising. Marc Bloch described the challenge this poses in a reflection on the demands of historical ecology that appeared within few years of M. A. Smith's pessimistic appraisal of the 'limitations of archaeological inference' (1955):

Now, if almost any important human problem ... demands the handling of diverse types of evidence ... the types of evidence necessarily mark off the several branches of technical scholarship. The apprenticeship for each is long, but full mastery demands still longer and almost constant practice. For example, very few scholars can boast that they are equally well equipped to read critically a medieval charter, to explain correctly the etymology of place-names, to date unerringly the ruins of dwellings of the prehistoric, Celtic, or Gallo-Roman periods, and to analyze the plant life proper to a pasture, a field, or a moor. Without all these, however, how could one pretend to describe the history of land use? (1953: 68)²⁴

In the case of Clarke's reinterpretation of Glastonbury, as well as the other examples discussed in this chapter, the catalogue of necessary expertise includes the resources of material science and physical geology, ethnographic and historical as well as archaeological comparanda, palaeoecology and agricultural science, as well as the skills of quantitative analysis and computer-based simulation that are now essential for the modelling approaches he advocated. The only way to ensure that the necessary range of expertise informs evidential reasoning in archaeology is to make archaeology a collaborative undertaking: a trading zone whose practitioners are sophisticated in

recruiting and applying a highly diverse array of external resources. This is the topic of our next chapter.

Finally, to return to earlier themes, Glastonbury and any number of other such cases illustrate the conclusion we drew from the long-running crisis debates about the paradox of material evidence. Viewed from a sufficiently high altitude of abstraction, the inescapable fact that evidence is theory-laden may seem to entail that everything is stacked against archaeologists ever being surprised by material traces; xeroxing is inescapable; pre-understandings determine that we will only ever reproduce expectations. But in practice, even when the fluid moment of interpretation ‘at the trowel’s edge’ is long past, it is often still possible to think against the grain, to bring new interpretive resources to bear, to challenge conclusions drawn and to reassess the framework assumptions that made possible the original ‘capture’ of the data that underpin evidential claims – and in this to put old data to work as evidence in new ways.

Notes

1

The terminology of ‘warrants’ for inference is drawn from Toulmin (1958), and the characterization of these warrants as ‘material postulates’ is based on Norton (2003); see Chapter 1.

2

This role of alternative hypotheses in evidential reasoning is suggested by Reiss’s (2015) pragmatic account of evidence discussed above in Chapter 1.

3

The term ‘secondary retrieval’ comes from Trouillot’s discussion of the production of history; for him it is the process of retrieving textual traces from an archive and configuring them as historical facts that can be built into historical narratives (Trouillot 1995: 8, 26).

This account of secondary retrieval, and that of recontextualizing (or ‘repositioning’) old data, is based on Wylie (2008, 2011b, 2014); the discussion of experimental modelling is developed in more detail in Wylie (2016).

4

Mississippian and Hopewell sites are attributed to distinct cultural traditions. The earlier Hopewell sites consist of earthworks and settlements ranging from 200 BCE to 500 CE (Middle Woodland) dispersed across the Northeastern and Midwest continental US. The occupants of these sites were horticulturists who cultivated Indigenous domesticates and participated in an expansive continent-wide trade network. Although the 'Hopewell Interaction Sphere' is made up of a number of distinct local and regional communities, their material culture is characterized by a distinctive regional design tradition and incorporates material traded from as far away as the Rocky Mountains and the Appalachians, the Gulf coast and the Great Lakes (Charles and Buikstra 2006). The later Mississippian sites date to 800–1600 CE and are characterized by elaborate ceremonial compounds that include earthworks and extensive palisades as well as mounds, a related design tradition – the Southern Ceremonial Complex – and well established practices of maize agriculture (King 2002, Pauketat 2004).

5

The Work Projects Administration (WPA) was a Depression-era federal relief programme that supported extensive excavation of archaeological sites throughout the US between 1935 and 1943.

6

This was a common practice in the period, and in other archaeological contexts. See, for example, Fulford, Clark, Eckardt and Schaffrey (2002) for discussion of Victorian excavation practice at the Roman town site of Silchester.

7

Despite the challenges, Connolly cautions against a tendency to dismiss the value of surviving records, and against interpretations that do not take into account counter-evidence available in the field notes and collections generated by earlier generations of archaeologists (personal communication with Wylie, 18 May 2006). Some of the field records produced for sites such as Fort Ancient in the 1940s are superbly detailed, but as the examples that follow indicate, valuable insights can be derived even from much less systematically documented excavations.

8

Schroeder observes that the WPA-era archaeologist who excavated Jonathan Creek made the assumption, common in the mid-twentieth century, that 'similar material traits between archaeological contexts and ethnohistoric and ethnographic descriptions reflected "common origins, history, and ethnicity," failing to recognize, as we do today, that evolutionary convergence and independent invention can produce material similarities' (2005: 64).

9

This was a crucial source of insight in the reanalysis of the collections created by Victorian excavators at Silchester (Fulford et al. 2002).

10

The cultural markers of distinctive stylistic traditions that appeared across a region were assumed to have diffused from dominant centres to smaller sites through lines of regional influence or migration.

11

As Sullivan puts this point, reflecting on the implications of her reinterpretation of the pottery sequences associated with the Hiwassee Island Mound, the ‘original interpretation of this site was highly influential’ – it was the anchor for region-wide interpretation of stylistic features first identified on this site – so that ‘making changes to those influential interpretations ... led to significant reinterpretations with far-reaching implications’ (2009: 205). Perhaps, she says, ‘eminent mound sites should ... be routinely reevaluated every few decades ... a “maintenance” program [that] would likely improve our interpretations as much as new, major excavations’ (p. 206).

12

A striking example of this principle at work is the re-examination of material recovered from the pits that make up the Aubrey Holes, a circle of pit features inside the bank and ditch of Stonehenge. A majority of these features were excavated from 1919 to 1926 by Richard Hawley. In 1935, archaeologists reburied in one of these pits, Aubrey Hole 7, the cremated human remains that had been recovered from a number of them; they record their location so that future archaeologists would know where to find them when they were in a position to bring to bear new analytical techniques. In 2008, permission was granted to re-excavate Aubrey Hole 7 as part of the Stonehenge Riverside Project. This made it possible to draw samples for radiocarbon dating that have the potential to settle a number of questions about the site’s chronology, initial layout and use, for example, establishing the Aubrey Holes as contemporary with the bank and ditch of the site’s original occupation (Parker Pearson 2012: 181–94).

13

In contrast to Schroeder’s reanalysis of legacy data from Jonathan Creek, in which archaeological data were the basis for challenging dominant WPA-era interpretive assumptions, the strategies illustrated here are a matter of appraising these warranting assumptions on independent grounds, and considering how the facts might be reinterpreted given alternative warrants. That said, Schroeder’s use of historical ‘genealogical’ analysis to make explicit

and problematize the theoretical framework that informed the WPA excavations grades into the strategies of recontextualizing legacy data that we describe here.

14

Several of the strategies Currie identifies in this connection – of triangulation and common process analysis – have long been a central feature of trace-theoretic accounts (Wylie 2016). They are strategies by which historical scientists extend the reach of trace evidence, bringing to bear indirect evidence of conditions that could produce surviving traces derived from general models of the type of system under investigation, and testing the implications of models of a target system to see if posited traces exist as predicted (Currie 2014b).

In addition, the use of models to simulate the causal interdependence between different aspects of a past system, process or set of events seems more plausibly an extension of methods of triangulation rather than the source of a novel type of evidence. The experimental manipulation of these models is informative only in so far as it can be assumed that the constituent elements of the system modelled were causally interdependent and claims about each of them have independent evidential support such that, if correct, they plausibly constrain what can be the case with respect to other aspects of the past. One of us has argued (Wylie 2016) that this practice of reasoning from explanatory coherence, a practice that Currie sees as a distinctive (non-trace-centric) strategy, depends upon an appeal to the convergence of distinct lines of evidence which is a standard feature of trace-centric accounts. That said, Currie brings these practices into sharp focus and teases out several dimensions on which the warranting assumptions about dependencies among variables and associated explanations can be assessed (Currie 2016).

15

See Wylie (2016) for discussion of an example of this practice: a suite of ‘how possibly’ models developed by Hegmon (1989, 1991) and Robertson (1997/2002) to explore the impact of different food-sharing practices on the survival rates of households in a small-scale Hopi-style farming community, and the potential for (some of) these practices to generate stratification.

16

The site itself was approximately 130 x 100 metres, with nearly 2 metres of deposits. Gray had been trained by Pitt Rivers and served as one of his assistants at Cranborne Chase.

17

In an analysis of archaeological modelling, Wylie (2016) draws on distinctions made in the philosophical literature between ‘phenomenological’ models – representational models of data (phenomena) that function as systematizing

tools – and explanatory models of the actual and hypothetical target events, systems, mechanisms and processes that generate these data. Clarke makes use of both types of model; his models of the relations that hold among elements of the primary database lie at the phenomenological end of this continuum and are representational. By contrast, his models of social organization are explanatory and they incorporate both representational and hypothetical elements; his aim seems to be to experiment with ‘the world of the model’, as Morgan describes this practice (2012), as much as to accurately represent ‘Iron Age society’.

18

These estimates of population are based both on formal models of the occupational space afforded by the combined house structures and on ethnographic and historical models of the size of a community that would be viable given the inferred subsistence base (p. 827).

19

He envisioned a monograph comparing the different interpretations of Glastonbury (Clarke personal communication with Chapman, 1975).

20

More recent analyses suggest that dry-land locations that could support domesticated animals and a plant-based diet were at least a kilometre to the south-east and north (Jay 2008). Clarke was clearly wrong in putting the emphasis on these resources in his land-use and economic models, rather than on wetland resources that would have been easily exploited in the immediate vicinity of Glastonbury.

21

To take another example of this objection that Clarke’s claims about internal structural patterns were unsubstantiated by the evidence or overextended, Coles and Minnitt (1995: 188) report that the analysed human remains from house floors provide no basis for Clarke’s (1972b: 839) interpretation of within-settlement exchange of women between extended families. Courbin described this as a ‘simple hypothesis [that] rapidly becomes a reality’ (1988: 32).

22

It is worth pointing out here that the Irish sources on Celtic society date from the Viking period, nearly a millennium later than Glastonbury.

23

Although C. Evans (1995) argues that it is questionable whether Coles and Minnitt’s present sufficient information to justify their alternative phasing.

We learned of this passage from Carole Crumley who quotes it, in her own translation, in the Foreword to *Advances in Historical Ecology* (1998: ix).

External Resources: Archaeology as a Trading Zone

Given the complexity of the cultural past as a subject of inquiry and the challenges of using material traces to study it, archaeologists must mobilize an enormously diverse array of warrants for their evidential claims. Of necessity, archaeologists recruit material postulates¹ from fields as diverse as nuclear physics and physical chemistry, palaeoecology and geology, material science and human physiology, social geography, cultural anthropology and ethno-history. No one domain-specific body of background knowledge will do the job. This is a daunting prospect. It requires the cultivation of discerning trading relations with a wide range of fields, and raises all the classic problems of evidence amalgamation. External partners have their own research agendas; often the questions they ask and the answers that interest them are wide of the mark when compared with the events and conditions of interest to archaeologists. Questions about what Toulmin (1958) refers to as the relevance, scope and strength of evidential arguments based on external warrants are ubiquitous, as are worries about the risks of nepotism when seemingly distinct lines of evidence are used to fine-tune one another; their convergence may be an artefact of adjusting each to conform to the others in the process of calibration rather than an indication that they provide different modes of access to a common target event or set of conditions. At the same time, the practice of building cables of mutually reinforcing and constraining lines of evidence brings with it distinct advantages by contrast, for example, to reliance on chains of inference that are only as strong as their weakest link (Wylie 1989, 2011b). Our central aim in this chapter is to tease out social/cognitive norms of discerning practice that can secure the epistemic advantages of triangulation, or ‘robustness reasoning’ as we describe it below, while avoiding the liabilities of spurious convergence among multiple lines of evidence in

the context of practice that is, of necessity, expansively inter- and multidisciplinary. These norms will turn out to be central elements of the procedural conception of objectivity that we identified in earlier chapters as a working ideal implicit in best practice, sometimes made explicit in critical debate about problematic cases.

We begin with a sketch of the complicated history of radiocarbon dating and the analysis of a test case published by Michael Shott (1992) midway through the process of its refinement that illustrates how hard-won its successful archaeological application has been. Radiocarbon dating is the classic example of an external resource that was expected, at the outset, to establish evidential warrants of unimpeachable scientific pedigree. The ambition was to build a body of evidence that could stand as an empirical foundation for absolute chronologies, secure in its own terms, warranted by material postulates drawn from physics that would decisively banish the 'element of conjecture' inherent in existing archaeological dating systems. In the event, the effective application of nuclear science to archaeological problems required an extended process of calibration, often against the very lines of evidence ^{14}C dating was meant to displace. This has been a process of iteratively refining inferential scaffolding that exemplifies a pattern of enthusiastic uptake, disillusionment and then painstaking refinement – a life history of technical imports to archaeology – that has been repeated many times over. Current wisdom is that the usefulness and credibility of external technical resources such as radiocarbon dating depend on bringing as many lines of evidence to bear as possible, not on securing any one of them as incontrovertible.

We then turn to two examples of research programmes that throw into relief the social, institutional scaffolding necessary for diverse imports and exchanges to succeed. One is the ongoing debate about the viability of provenance analysis that depends on tying metal artefacts to the ore deposits from which their raw materials originated. A crisis of confidence in the use of lead isotope analysis in the UK generated a probing reassessment of the orienting questions about provenance, as well as a systematic retooling of the warrants that mediate the use of provenance data as archaeologically relevant. The second, which we treat more briefly, is the 'Diaspora

Communities' project in which sustained interdisciplinary teamwork made it possible to mobilize a number of different lines of evidence to address questions about the origins and lifetime travels of people interred in Romano-British cemeteries. We characterize these as evolving trading zones, drawing on Peter Galison's (1997) account of how scientific communities navigate problems of inter-field communication when trading partners operate with very different modes of research practice and specialist languages that are to some degree incommensurable (2010: 25–9; 1997).²

Together these cases illustrate several norms of critical engagement that we believe are important conditions for productive epistemic iteration. In characterizing them we draw inspiration from Helen Longino's description of four such social/cognitive norms, the cornerstone of her account of procedural objectivity, but we reconfigure and amend these in light of insights specific to these cases (2002: 128–31). In Longino's account, well functioning research communities must maintain practices that subject contending beliefs to 'criticism from multiple points of view' and they must do this in a way that secures the possibility of 'transformative criticism' (1990, 73–4) of their research agendas and framework assumptions as well as of the specific claims under investigation (p. 129). Her guidelines for such practice include the requirements that there be (1) public venues for criticism which ensure that dissent can be voiced, and (2) norms of uptake for criticism; that (3) the standards that inform criticism are publicly recognized and themselves open to critical assessment, and that (4) practices of critical engagement should embody a norm of 'tempered equality of intellectual authority' (p. 131). This last means that critical input should not be deflected on the basis of social indicators:³ 'not only must potentially dissenting voices not be discounted; they must be cultivated' (p. 132). To fail to fulfil these 'duties of inclusion and attention', she argues, is 'not only a social injustice but a cognitive failing' (p. 132); it compromises the epistemic integrity of the research community and the claims it ratifies as knowledge. For present purposes we think of these norms in terms of requirements of transparency (Longino's first and third norms) and of responsiveness to criticism (Longino's second and fourth norms). We make explicit an implication of these norms for evidential reasoning;

although some assumptions and claims must be accepted for purposes of furthering a line of inquiry – ‘respected’, Chang would say (2004: 2225), as the necessary scaffolding for inquiry – none should be presumed foundational in the sense that they are self-warranting or otherwise exempt from critique. Finally, we emphasize a point that archaeological practice makes abundantly clear: effective critical engagement requires that widely diverse types of expertise be brought to bear. It follows from this that Longino’s fourth norm – the requirement to recognize intellectual authority – extends well beyond the bounds of the archaeological research community and that the tempering clause must be formulated in terms that allow for a nuanced assessment of the credibility and relevance of diverse types of external expertise.

The multiple radiocarbon revolutions

Without the successful development of chronologies, the nineteenth century would not have witnessed the emergence of the discipline of archaeology. Conceptual schemes proposing a succession of prehistoric ‘ages’ were founded on typologies that capture patterns of association among artefacts found buried together (e.g. in burials and hoards, Trigger 1996: 124, Rowley-Conwy 2007: 32–47), seriations that document the orderly succession of form and design within classes of artefacts (e.g. Deetz and Dethlefsen 1967), and the stratified deposition of this material (e.g. Gräslund 1987). Archaeologists relied heavily on historical chronologies to anchor archaeological sequences when links to textual or epigraphic records could be made, but it was the process of ‘learning to see’ chronological structure in the archaeological record that Bruce Trigger, for example, has hailed as a distinctively archaeological breakthrough (1996: 135–7). These archaeological chronologies were worked and reworked, extended and subdivided, filling the professional literature with periodizations, cultural groupings, highly detailed studies of artefact types and classes, and weighty corpora of such artefacts. External technical resources played a role in constructing and refining these systems – for example, stratigraphic analysis depends on geological principles (Renfrew 1973: 24) – and archaeologists made effective use of tree-

ring and varve analysis to build absolute chronologies of limited scope (pp. 26–7). But for the most part the dating of archaeological material was an internal affair until the arrival of radiocarbon dating.

When first introduced, there was enormous enthusiasm for the prospect that radiocarbon dating would solve a range of chronological problems in archaeology, supplanting dependence on the insecure, local and relative chronologies on which archaeologists had long relied.⁴ It was the physical chemist Willard Libby who recognized that the rate of decay of radioactive carbon isotopes might be exploited for archaeological purposes; he had worked for the Manhattan Project that produced the first nuclear weapons during the Second World War, and received the Nobel Prize in 1960 for his post-war work developing this particular peace-time application of nuclear physics. He particularly emphasized the stability of the physical process of radioactive decay as the crucial warrant for its use as the anchor for archaeological dating:

The rate of disintegration of radioactive bodies is extraordinarily immutable, being independent of the nature of the chemical compound in which the radioactive body resides and of the temperature, pressure, and other physical characteristics of its environment. (Libby 1952: 9, as cited by Francis 2002: 297)

Libby's insight was that, given this stable decay rate, if you know the ratio of radioactive (^{14}C) to stable carbon (^{12}C and ^{13}C) in the atmosphere in which a sample of organic material originated you can determine when it ceased to exchange carbon with the atmosphere; the difference between the proportion of carbon in the sample and the baseline ratio makes it possible to estimate the time elapsed since the decay process began.

The radiocarbon revolution that Libby set in motion has, indeed, been 'sensational', as Sturt Manning puts it; the results have 'entirely restructured the practice and understanding of prehistoric archaeology around the world' (Manning 2015: 128). However, realizing its promise has not been straightforward; in Manning's account this has required two subsequent radiocarbon revolutions. By the late 1950s, questions were being raised about the reliability of ^{14}C results and within a decade it was clear that radiocarbon dating could not be treated as a 'silver bullet'. To begin with, it took some time for

radiocarbon laboratories to refine methods for measuring ^{14}C in archaeological samples that control for the effects of electromagnetic impurities, ambient radiation, radon contamination and fractionation (in reactions that do not go to completion), and to standardize count-time and conventions for calculating and reporting error. By the early 1990s, protocols ensuring inter- and intra-lab reliability had been instituted,⁵ and archaeologists had established procedures for minimizing contamination by younger or older organic material when recovering and handling samples. But in the process, and as radiocarbon dating was increasingly widely used in archaeological contexts, a number of anomalies were identified that could not be attributed to contamination or processing error and that made clear just how complex the physical processes are that underpin the method. It was this realization that catalysed the second revolution: the long process of calibration that began in the mid-1960s (Manning 2015: 129).

It was discovered early on that Libby's original estimate of the half-life of ^{14}C was out by 162 years; improved estimates available by the late 1960s set it at 5730 ± 40 years rather than 5568 ± 30 years (Renfrew 1973: 288), but for pragmatic reasons it persisted as the standard long after the correction was made.⁶ The most significant insight where archaeological applications are concerned was, however, the growing realization that the proportion of ^{14}C to ^{12}C and ^{13}C in the atmosphere is not uniform over time or space, or in its uptake by different types of organic matter. The industrial and bomb effects are particularly strong; 160 years of burning fossil fuels has dumped steadily growing amounts of 'dead' carbon into the atmosphere, depressing the proportion of radioactive to stable carbon isotopes, while above-ground nuclear tests in the Cold War era dramatically increased the proportion of ^{14}C (Gillespie 1986: 20). Even when these effects are controlled for, samples from different types of organic material have different concentrations of ^{14}C depending on whether they are terrestrial or marine (e.g. whether they absorb carbon in the form of bicarbonate rather than carbon dioxide, or occur in carbon sinks created by ocean currents), what kind of photosynthetic pathway they use to fix carbon (this differs between arid, succulent or temperate zone plants) and whether their

metabolic processes discriminate against heavy isotopes (e.g. in bone collagen). So, from the outset, there was a very real question about what standard to use as the atmospheric baseline for determining how long the ^{14}C in a particular sample had been decaying since it stopped exchanging carbon with its environment. This was initially Cretaceous carbonate (Pee Dee Belemnite, PDB) and subsequently oxalic acid corrected to the average count rate for terrestrial wood dating to 1950. The discussion of this point in the Oxford *Radiocarbon User's Handbook* of 1986 is particularly interesting:

The choice of this value [the average value for terrestrial wood used to normalize the measured carbon-14/carbon-12 ratio] is arbitrary, and other values could have been used with perhaps more theoretical justification. This normalization procedure, however, has been agreed internationally by the radiocarbon community, and the user is encouraged to check whether the laboratory does in fact use it. (Gillespie 1986: 18)

In addition, ^{14}C production is affected by sunspot activity and dipole movement,⁷ and these effects are sometimes amplified by associated changes in temperature that have an impact on the atmospheric mixing and circulation of ^{14}C as well as its rate of absorption into carbon reservoirs. The result was recognition by the early 1980s that there are global differences in the concentration of ^{14}C between the northern and southern hemispheres, given proportionately more ocean surface in the southern hemisphere (this allows for more rapid transport of ^{14}C into ocean reservoirs) and also local variation that results from geological events such as volcanoes and geysers or in cases where climatic factors affect the rate of ^{14}C exchange between atmosphere and ocean (Browman 1981: 249–67; Gillespie 1986: 26–7). A series of reports that appeared in *Science* in 2001 documented ‘a regional, time-varying ^{14}C offset [that] can occur within a hemisphere’ (Kromer et al. 2001, Manning et al. 2001, Reimer 2001), in this case in securely dated tree-ring samples from Anatolia and southern Germany that grew at the same time (fifteenth to seventeenth centuries CE). The authors hypothesize that this is a consequence of a solar minimum which raised ^{14}C levels, depressing radiocarbon relative to calendric ages, and an associated cooling effect that had a seasonally different impact on trees characterized by different growth periods (Kromer et al. 2001: 2529–30; Manning et al. 2001: 2533).

Identifying, measuring and building these effects into estimates of radiocarbon dates is an ongoing process.

In short, the inferential warrants necessary to make effective use of the ^{14}C decay rate as a basis for archaeological dating are immensely more complex than initially realized by advocates of the first radiocarbon revolution. What Manning describes as the second radiocarbon revolution has been a long process of calibrating the atmospheric ^{14}C dates for samples of known age and reconciling discordant chronologies that has required expertise culled from dozens of widely disparate fields. Initially the basis for calibration was tree-ring data, but it has also included artefact sequences, stratigraphic data and historical records, just the kinds of chronological evidence that radiocarbon dating was expected to displace. The work of refining and integrating regional calibration curves is ongoing and is regularly reported at international radiocarbon conferences and in special issues of the journal *Radiocarbon*,⁸ and several systems for calibrating ^{14}C dates are available online (e.g. CALIB 7.1, Stuiver, Reimer and Reimer 2016; OxCal 4.2, Bronk Ramsey 2015). As these were refined, ‘wiggle effects’ were identified such that, for some periods of archaeological interest, samples with different true ages correspond to the same radiocarbon ages, or the spread in their true ages is exaggerated, compressed or even reversed. This reinforced the now conventional wisdom – the catalyst for the third radiocarbon revolution – that, in any archaeological application of the method, radiocarbon results must be interpreted in light of other contextual and chronological evidence.

These implications for archaeological dating were articulated by Michael Shott (1992) in terms of a small-scale test case at a point when the second revolution was in mid-stride. He considers a puzzling suite of ^{14}C dates for the Childers Site, a Late Woodland site in the Ohio Valley; the radiocarbon dates available at the time suggested that the site was occupied for 600 years (1050–1650 BP) with a discontinuous later occupation of 200 years (750–950 BP), while the archaeological evidence suggested that Childers was the result of a single relatively short occupation of ten to fifty years sometime in the period 400–800 CE (Shott 1992: 204, 207), results that, ‘at face value ... resist simple interpretation’ (p. 208). Rather than take the physics-

backed radiocarbon dates as given, Shott scrutinized each line of evidence, assessing its security in its own terms and its plausibility in relation to the others (see Fig. 4.1).⁹ He started with the radiocarbon samples, rejecting a third of them on grounds of poor provenance or risk of contamination, and then developed several strategies for evaluating the competing hypotheses about the Childers occupation suggested by the ^{14}C results and the archaeological evidence. The pivotal insight here is that what radiocarbon analysis dates is a natural event – the point at which organic material stopped exchanging carbon with the atmosphere – which may not coincide with the cultural event that archaeologists are intent on investigating (p. 203; see also Chippindale 2002). Given cultural practices of reuse, curation, trade and other forms of circulation, the organic samples that archaeologists date may have been burned or cut long before they were deposited in the contexts from which they are recovered. This problem of ‘old wood’ (Schiffer 1986) requires close attention to the context and association of the dated samples (Shott 1992: 203). This is a matter not only of controlling for risks of contamination but of using what Shott refers to as ‘independent evidence concerning a site’s antiquity’ to determine how radiocarbon results should be interpreted in relation to the cultural events of archaeological interest (p. 203).¹⁰

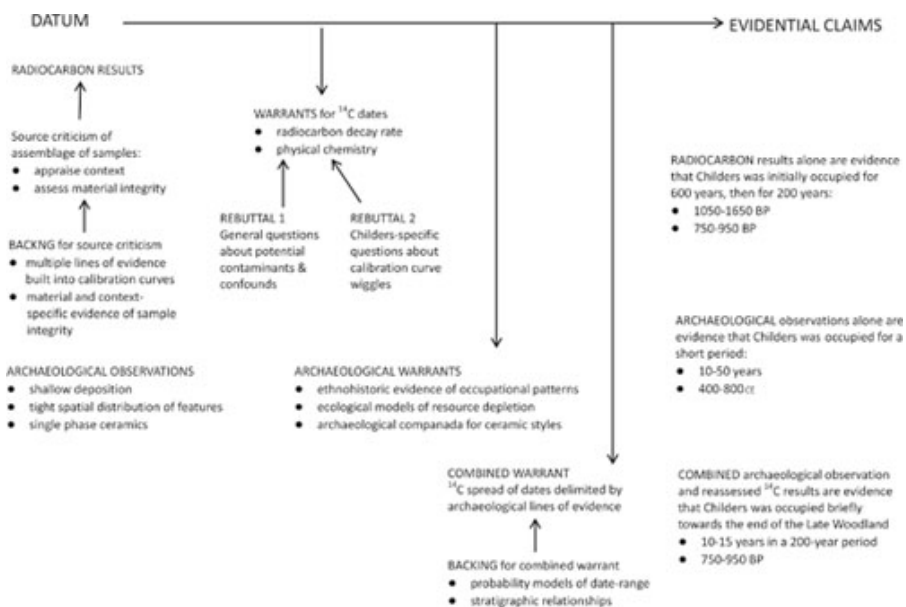


Figure 4.1 The Childers site chronology: multiple lines of evidence with diverse warrants and backing, laid out according to Toulmin's argument schema (see Fig.

The archaeological evidence for a single, relatively short occupation includes characteristics of the site itself – the relative homogeneity of assemblages in all major classes of cultural material and the low rate of feature overlap compared with other Late Woodland sites and assemblages – as well as background knowledge about the rates of decay typical for the types of wood used in construction at the site, patterns of resource depletion associated with the foraging and horticultural activities documented for Childers, and ethnohistoric evidence for regional trends in the typical span of occupation for sites in the region. Although these lines of evidence establish no precise length of occupation for Childers, they do reinforce Shott's initial conclusion that Childers was not occupied for anything like as long as, or in the discrete periods suggested by, the radiocarbon results.

Shott's reanalysis of the radiocarbon dates includes pair-wise tests for contemporaneity, the calculation of a mean occupation date and measures of dispersion from the most credible radiocarbon results (calibrated to 585 CE), and also a strategy of modelling the dispersion of dates that, given standard sources of error, could be generated by samples that originated in a ten- to fifty-year occupation whose hypothetical true date is the average suggested by the pooled radiocarbon dates. This last suggests that the wiggle effects built into calibration curves for the period in question, and the implications of normalizing ^{14}C dates for the kinds of material that make up the Childers samples, could well produce radiocarbon dates that range over 200 years for samples with contemporaneous cutting or burning dates. Shott concludes that the dispersal of dates for the most reliable radiocarbon assays is consistent with the archaeological hypothesis for Childers; the samples could all have originated in a single short ten- to fifty-year occupation, as the archaeological evidence suggests, but most likely towards the end of the Late Woodland period in the 200-year date range suggested by the ^{14}C results.¹¹ This means that, although 'reasoning and inference from the radiocarbon results override our prior beliefs about the site's age' (p. 219), when the radiocarbon dates are interpreted in light of the archaeological, ethnohistoric and ecological data, the combination of these lines of evidence 'warrants archaeological conclusions that an uncritical

reading of all radiocarbon results would not support' (p. 225). Given that 200 years is as close a determination of the occupation dates for Childers as radiocarbon dating can be expected to yield – at the time, for this period and for the types of sample analysed – Shott urged archaeologists to redouble their efforts to refine and extend existing local and relative chronologies – in this case, chronological sequences based on the seriation of ceramics and other classes of tools and artefacts. Reflecting on the 'vagaries' of calibration, he sees this as 'a method that not only controls the time dimension', reducing reliance on radiocarbon dating, but that also 'tracks subtle cultural variation' (p. 226); it has the potential to generate evidence relevant to cultural events to which radiocarbon dating is not necessarily sensitive.

Shott's assessment of radiocarbon dating at this juncture (1992) is cautiously optimistic; he acknowledges that its importance for archaeology 'is almost impossible to exaggerate', but observes that it had 'failed to meet the high expectations we developed for it' (p. 202). The accumulated wisdom of the second revolution is reflected both in his appraisal of the limitations of radiocarbon dating and in his recognition that major technical advances had been realized in the recovery and processing of ^{14}C samples, as well as in the use he made of the growing body of background knowledge that underpins the calibration of radiocarbon dates to appraise sample integrity and margins of error. Shott was by no means alone in both embracing and urging caution with respect to archaeological applications of radiocarbon dating. Contention was sharpest in Old World contexts where the theoretical stakes were especially high. In *Before Civilization* (1973), Colin Renfrew made much of the implications of rapidly accumulating ^{14}C dates that, by the early 1970s, seemed to decisively challenge conventional archaeological wisdom. Long-standing regional chronologies based on artefact sequences or historical sources were 'up-ended', as Manning puts it (2015: 133), exposing chronological fault lines which called into question diffusionist explanations that accounted for pre- and proto-historic cultural change across Europe and around the Mediterranean in terms of a core region in Egypt or the Near East (Manning 2015: 133, Renfrew 1973). However, as more precise calibration curves were developed and accelerator mass spectrometry (AMS) dating of much smaller samples became

available, the reversals hailed by Renfrew were complicated by new anomalies; in some cases conventional chronologies proved to be compatible with calibrated ^{14}C dates (e.g. for the early and late Bronze Age in the Aegean and in Cyprus; Manning 2015: 139); in others, new gaps and discrepancies were identified that complicated the culture-historical picture for all parties to these debates.

Looking back on the history of the second revolution debate, Manning notes a crucial shift in approach marking the advent of a third radiocarbon revolution which was catalysed by growing recognition that the problem of dissonant chronologies could not be resolved solely by further refining the calibration of radiocarbon sequences. As Shott found in the microcosm of the Childers site, establishing culturally relevant as well as secure and precise absolute dates requires 'archaeological observation and judgment' (1992: 203). Citing examples in which, in the last decade, 'new data have crystalized the issues, and in several cases narrowed or clarified gaps' (2015: 130–40), Manning observes that:

The truly notable aspect of recent work ... is that sophisticated ^{14}C analysis, integrated with the archaeological evidence, is now being used by all sides in many debates as the primary basis for chronologies (and as the framework of history) – even inside the area of Renfrew's supposed chronological faultline – as opposed to archaeological linkages with Egypt. Thus European-Mediterranean archaeological timescales have been reunified for the first time since the 1950s (another revolution). (p. 140)

The impact of this continuing second revolution has been dramatic, and constructing the necessary scaffolding for calibration, in the form of background knowledge drawn from fields as diverse as molecular biology, botany, geochemistry, environmental science and astronomy as well as archaeology itself, has required a sustained international effort to establish common standards and practices. It has been a matter of building a complex, mutually stabilizing network of skill and instruments, theoretical and factual knowledge of the kind that Ian Hacking describes, in the context of experimental research, as a consilient network of 'ideas, things, and marks' (1992: 44). The creation of this robust trading zone is a social and institutional as well as an empirical and theoretical accomplishment. But increasingly the successes of radiocarbon dating are being realized not by refining the

warrants that back radiocarbon dating, but by expanding this trading zone.

The third radiocarbon revolution focuses attention squarely on the challenges identified by Shott in the early 1990s. It is animated by two now widely accepted insights that he articulated in his analysis of the Childers site. The first is the prosaic point, already made, that however precise radiocarbon analysis is, it dates a natural event so that its use in archaeological contexts requires a series of inferences about how this event relates to the cultural contexts and events of archaeological interest.¹² And the second is that radiocarbon dates are probabilistic; their use in an archaeological context requires archaeologists to model a spectrum of dates that the originating event could have generated given the factors that may have affected the ^{14}C signature recorded for a given sample. This reinforces the wisdom that, rather than seeking the certainty of a physics-backed line of evidence that can displace reliance on archaeological chronologies and their ‘web’ of background assumptions, the challenge is to ‘fully integrate archaeological information with ^{14}C dating in order to address archaeologically relevant (and therefore socially relevant) timescales and episodes’ (Manning 2015: 151).

The distinctive contribution of the third radiocarbon revolution is the development of systematic analytical techniques for using multiple lines of evidence to assess margins of error in physical dating, and delimit, within the range of physically possible dates, a subset of archaeologically plausible dates. These include strategies of secondary retrieval and source criticism of the kind that Shott used to assess the provenance and integrity of samples from which ^{14}C dates are drawn, as well as the use of stratigraphic data, design sequence seriations, typological convergence and spatial distributions to generate a set of chronological models for the target event or context of archaeological interest. Sensitivity analyses are then applied to these models to test their robustness:

One component of a model is changed, and the model is rerun. The outputs from the original model and its variant are then compared. When these are very similar, then the model can be regarded as insensitive to the component of the model that has been varied. When the outputs differ markedly, the model is sensitive to that component. Sensitivity analyses are useful not only in determining how far the outputs of a model are stable, but also help us to

identify which components of a model are most critical to the resultant chronology. (Bayliss and Whittle 2015: 234)

Alex Bayliss and Alasdair Whittle argue that these strategies of triangulation can be applied to cases ranging from highly local, short-lived homestead occupations, like the Childers case that Shott analysed, to large-scale landscapes and millennial timescapes such as the new Stonehenge chronology (Parker Pearson et al. 2007) which challenges the conventional assumption that timber circles must have predated the famous stone monuments.

The pivotal insight here is that there is an ‘element of conjecture’ inherent in all evidential claims,¹³ not only because the inferences that link them to anchoring facts routinely fall short of deductive certainty, but because these empirical anchors are themselves inferential constructs, dependent upon a scaffolding of warrants and assumptions that are open to question. While this suggests that the quest for incontrovertible empirical foundations is misguided, it does not follow that anything goes. Bayliss and Whittle emphasize the potential for diverse lines of evidence to both constrain and reinforce one another, describing their advocacy for integrating multiple lines of evidence as a ‘pragmatic Bayesian’ approach. In a philosophical analysis of the role of Bayesian statistical reasoning in radiocarbon calibration, Dan Steel (2001) describes archaeologists as ‘eclectic and pragmatic’ in their use of statistical tools (p. 154); for practical reasons of computational tractability as well as substantive reasons to do with the irregularity of calibration curves, packages such as CALIB make use of Bayesian statistics alongside classical statistics (p. 162).¹⁴ But in addition to this technical application of Bayesian techniques to second-revolution calibration problems, Bayliss and Whittle (2015; Bayliss et al. 2007) characterize the third revolution as Bayesian in a more informal sense. They find the rationale for their triangulation strategies well articulated by the requirements, central to Bayesian models of confirmation, that any assessment of the bearing of (new) evidence on a hypothesis must take into account how well supported the hypothesis is on other grounds (its prior probability), as well as the degree to which the evidence in question is discriminating: whether it would hold regardless of the truth or falsity of the test hypothesis (an appraisal of the prior and posterior likelihood of the

evidence cited).

Construed in pragmatic terms, the Bayesianism of the third radiocarbon revolution is a classic example of the use of methods of ‘multiple determination’ (Wimsatt 1981: 123–4; Soler 2012: 3) to which many practitioners and philosophers attribute the success of the empirical sciences. William Wimsatt has influentially described a broad array of such practices as various forms of ‘robustness’ reasoning that serve to establish ‘the existence and character of a common phenomenon, object, or result’ (p. 123), the reliability of the instruments and systems of measurement used to detect and to probe these phenomena, and the models built of them (Wimsatt 2012: 93–4). In an analysis of microscopy, Ian Hacking describes a subset of these practices that closely parallel pragmatic Bayesian practice in archaeological dating (Hacking 1981; 1983: 186–203).¹⁵ He argues that we believe what we see through optical, acoustic and scanning electron microscopes not just because each of these instruments depends on well understood physical principles – evidential claims about the entities observed are backed by inferential warrants that render them individually secure¹⁶ – but because, when used in conjunction with one another, it is implausible that images produced by such different means would converge as a consequence of confounding influences that generate compensating error in each of these very different lines of evidence. By extension, when multiple lines of evidence fail to converge, they have a capacity to expose error that might not be detected in an assessment of the security of the backing and inferential credibility of each taken on its own. In an archaeological context, Wylie has described this as a matter of building cables rather than chains of evidential reasoning (1989). The principle at work here is that evidential reasoning is at its strongest when archaeologists can exploit the epistemic independence of distinct lines of evidence that rely on causally independent processes of trace generation and on conceptually independent detection techniques and inference-warranting bodies of background knowledge.¹⁷

This rationale for robustness reasoning has been described as a type of ‘no-miracles’ argument – ‘it would be miraculous if multiple independent experiments showed *x* (where *x* is an entity, or a process,

or a constant, or a relation) and x was not real' (Stegenga 2009: 653) – and it has recently been challenged by philosophers of science who are sceptical of Wimsatt's more ambitious claims about the epistemic virtues of convergence (e.g. Stegenga 2009: 654–6, Hudson 2014; see also Soler 2014). However this philosophical debate about the distinctiveness and ubiquity of robustness reasoning is resolved, it has directed the attention of both critics and advocates to two sets of reasons for caution about robustness reasoning. One is a concern that the rhetorical force of convergence arguments can be misleading; convergence may add little epistemic weight to that provided by distinct lines of evidence considered on their own, or by a decisive line of evidence that supersedes the evidence built into its scaffolding.¹⁸ Another is that, although Wimsatt and others emphasize the importance of robustness reasoning as a means of exposing and controlling for observational and analytic artefacts,¹⁹ in practice the processes of calibration and mutual adjustment required to integrate diverse lines of evidence carry a very real risk of artificially producing convergence. Taken together, these objections suggest a number of conditions that must be met if the risks of spurious convergence are to be avoided, all of which figure prominently in archaeological debate about the robustness of evidential reasoning:²⁰

1.

Security: each line of evidence (its warrants and their backing) must be credible in its own right.

2.

Causal anchoring and causal independence: for a suite of evidential claims it must also be demonstrated that the material traces anchoring them are causally produced (in the first instance) by the same target of inquiry but that their subsequent transmission is causally independent.

3.

Conceptual independence: the warrants backing the interpretation of each line of evidence must also be independent. In particular, they must not depend on common assumptions (implicit or explicit) that could produce artificial convergence in the interpretation of different anchoring 'facts' (data) as evidence.

4.

Grounds for calibration: the calibration of warrants backing each line of evidence must be justified on independent grounds, not because they ensure the convergence of these lines of evidence.

5.

Addressing divergence: when lines of evidence fail to converge, each must be assessed for sources of error in their warrants and the backing for these warrants; no one line of evidence can be assumed secure and exempted from critical scrutiny.

The trajectory of the multiple radiocarbon revolutions can be read as a sustained process of addressing concerns arising from these conditions. The initial enthusiastic reception of radiocarbon dating reflected confidence that, given its backing by nuclear physics, it met the first condition with a vengeance: it seemed uniquely secure. So long as it could be assumed that radiocarbon dates are determined (exclusively) by the invariant decay rate of ^{14}C , it was plausible that the method could deliver a universal, non-contingent evidential foundation for dating archaeological material. There was no need to rely on multiple lines of evidence except when questions of relevance arose about the bearing of radiocarbon dates on the cultural events of archaeological interest, and there seemed no question but that the second two conditions of causal and conceptual independence were met. The physical processes that give rise to a distinctive, time-sensitive ratio of radioactive to stable carbon in a sample of organic material are, in an obvious sense, causally independent of the cultural and material processes that produced and preserved the sample in an archaeological context. Moreover, the background knowledge from nuclear physics on which radiocarbon dating depends could be assumed to play no role in the construction of archaeological chronologies anchored in historical records, stratigraphic data or stylistic seriation. Combined with the assumption of unimpeachable security, these considerations underwrote the expectation that radiocarbon dating could (and should) supplant reliance on local, contingent, conceptually entangled chronologies based on archaeological and historical evidence.

The catalyst for the second radiocarbon revolution was initially a set of concerns about whether, in fact, radiocarbon dating met the first condition when it was recognized that a great many factors other than

Libby's decay rate affect the measured proportion of stable to radioactive carbon in archaeological samples. Establishing the security of this singular line of evidence put a premium on strategies of secondary retrieval and source criticism by which the anchoring facts are scrutinized, and on the painstaking process of building and scrutinizing the warrants that underpin the inference of evidential claims from these facts. Conceived as a process of calibration, this was, in the first instance, a matter of identifying alternative lines of chronological evidence that are sufficiently secure in their (limited) domains of application that they can be used to cross-check radiocarbon dates. This, in turn, requires that the second and third conditions are met: these distinct lines of evidence must be shown to originate in the same target event, but to follow causal pathways that are not affected by the same sources of distortion and that depend, for their interpretation as evidence, on conceptually distinct ranges of background knowledge. Dendrochronology seems to meet these conditions straightforwardly: the annual growth of tree-rings might be distorted by climatic fluctuations but not by the factors that affect the decay rate of radioactive carbon, so comparing the count of growth rings with the ^{14}C date for a well preserved sample of wood should provide just the kind of causally and conceptually independent control required, at least for some stretches of radiocarbon-based chronologies. This picture becomes complicated, however, when it is recognized, for example, that sunspots not only have an impact on the baseline ratios of stable to radioactive carbon in the atmosphere but also can affect climate that in turn affects the growing period and growth rate of trees. In the process of assembling the scaffolding of warrants necessary to calibrate radiocarbon curves it became necessary to interrogate and substantiate the assumptions of independence that had underwritten the optimism of the initial revolution. The credibility of the calibration curves developed over decades of painstaking international, cross-field collaborative work ultimately depends on meeting the fourth condition: bringing into play background knowledge about the nature and effects of potential confounds and their interaction – for example, knowledge of the production, sequestration and uptake of atmospheric carbon drawn from fields as diverse as atmospheric and marine science,

palaeoecology, biochemistry and botany – that justify fine-tuning the interpretation of radiocarbon results but do not already figure in the generation of these results.

As the security of radiocarbon dating improved, the need to address the relevance component of the second condition – establishing how the natural events dated by means of radiocarbon analysis relate to the cultural events that archaeologists investigate – came sharply into focus. In the context of the third radiocarbon revolution, multiple lines of evidence are used not just to calibrate radiocarbon dates but as an essential resource, alongside ^{14}C dates, for building archaeological chronologies. Pragmatic Bayesians are explicit about this: what they advocate is a practice of genuine robustness reasoning in which no one line of evidence stands as a uniquely secure empirical foundation for answering questions about the ‘time dimension’ of the cultural past (Shott 1992: 226). Considered in this light, the insight central to the third radiocarbon revolution is that robustness is by no means miraculous; it is the product of hard work on multiple, irreducibly local but widely networked fronts. The central challenge has been to cultivate the cross-border expertise necessary to sustain a well functioning trading zone; we refer to this in what follows as interactional and meta-expertise, following Harry Collins and Robert Evans (2007).²¹ The often remarked pattern of boom and bust cycles in the adoption of external resources by archaeologists reflects not only the grip of a tenacious, if implausible, epistemic ideal – that genuine knowledge, knowledge worth the name, must be warranted by absolutely secure (foundational) facts and deductive inference from them – but also the reality that it is extremely difficult to build research programmes that meet the conditions outlined here for virtuous, non-circular, robustness reasoning. We turn now to consider two examples of cross-field trade that illustrate where the pitfalls lie in effectively integrating external resources into archaeological research programmes, and what is required to establish functioning trading zones that meet these conditions in practical terms.

Metals moving: Lead isotope analysis

The first of these cases is something of a negative object lesson: it is

the troubled history of debate in the UK about the use of lead isotope analysis (henceforth LIA) to establish the locus of origin and movement of Bronze Age copper artefacts. Although the analytic techniques that make it possible to characterize copper objects and ore deposits in terms of their trace element profiles were not available until the 1930s (Pernicka 2014), Mark Pollard and Peter Bray (2015) trace an interest in the chemical composition of metal objects back to analyses of the contents of cabinets of curiosity in the eighteenth century. Early studies focused on identifying which alloying metals had been used in ‘ancient’ artefacts (p. 114), but by the mid-nineteenth century, attention had turned to questions about the geographical sources of these metals and increasingly chemical analysis was seen ‘as a proxy for “contact” (however that term might be construed) between peoples in the past’ (p. 115). With this, Pollard and Bray argue, the ‘quest for provenance’ was firmly entrenched and, despite significant changes in the analytic techniques available, ‘little has changed theoretically in the last 150 years’ (p. 115). After the 1930s, chiefly in Germany, Austria and the Soviet Union, new techniques of trace element analysis seemed to bring within reach the possibility of identifying a ‘chemical or isotopic fingerprint’ that could be used, alongside archaeo-metallurgical fieldwork, to link prehistoric metal objects from the Chalcolithic and Bronze Age periods to specific ore deposits. The working assumption, as Pollard and Bray describe it, was that there should be ‘some characteristic of the source of the raw material that passes through to the finished object and allows a relationship to be established between the two’ (p. 116).

Promising as this research programme seemed, a number of critical papers from the UK and north-west Europe put into increasing doubt the ability of established methods of trace element analysis to tie metal artefacts to ore deposits with any degree of confidence (e.g. Tylecote 1970; Slater and Charles 1970). English-speaking archaeologists had lost faith in trace element analysis by the early 1980s (e.g. Coles 1982) and looked to invest in a more reliable analytical technique for determining prehistoric metal provenance. Lead isotope analysis had been introduced to archaeology in the late 1960s, initially to determine the provenance of the metals used in gold and silver artefacts,²² but from the early 1980s it was redirected

to the analysis of copper artefacts, chiefly with the aim of better understanding their role in second-millennium BCE Mediterranean trade networks. We focus here on debate generated by the LIA research programme associated with the Isotrace Laboratory at Oxford²³ as a particularly dramatic example of the difficulties and controversy that often accompany the lifecycle of adoption, critique and, in this case, nearly complete abandonment of external technical resources in archaeology.

When LIA analysis was first applied to copper artefacts and ore deposits, expectations of what it could deliver were high. Given the narrow range of variation in the lead isotope content of ore bodies, and initial evidence of correspondence between these values and those of tested artefacts, it seemed that LIA would bring within reach the ambition of ‘fingerprinting’ ore deposits with a level of resolution that could be used to address questions about provenance and trade relations.²⁴ From the outset, however, it was acknowledged that, given the nature of the lead isotope profiles of known ore deposits and artefacts, the Oxford research group would have to address the challenge that LIA analysis serves most reliably to exclude attributions of origin and to establish the isotopic ‘consistency’ of artefacts with ore deposits. It can rule out ore sources when their isotopic profile is clearly not a match with that of a particular artefact, but, on its own, it does not establish a ‘post-code’ identification of an artefact with a unique ore source. The Oxford group characterized known ore deposits in the Mediterranean/Aegean study region in terms of a ‘field’ of ten to twenty lead isotope values using stepwise discriminant function analysis, but even with this degree of specificity the possibility remained that documented deposits of similar geological age could have overlapping lead isotope profiles, and that their values might overlap with as yet unknown ore deposits that had been exploited in antiquity. None the less, the use of archaeological evidence alongside trace element analysis could strengthen the likelihood that metals originating in the same ore deposits were used to produce widely dispersed artefacts, and overlaps between the signatures of artefacts and ore deposits do raise the likelihood that an ore deposit had been exploited in antiquity. So the principals of the Oxford group, Noël Gale and Zofia Stos-Gale, initiated field surveys of

ore deposits in the Aegean, collecting samples for analysis and looking for evidence of exploitation in later prehistory as well as in historical times, at the same time as they took samples from Bronze Age artefacts to analyse for LIA profiles. Expanding over study areas in the eastern, central and western Mediterranean, their overarching goal was to answer questions about the extent and scale of Bronze Age trade networks in metals and metal objects.²⁵

The biggest surprise of the Oxford group's research programme came from the analyses of Late Bronze Age copper oxhide ingots from shipwrecks and land finds extending from the Levant through Cyprus, Turkey, the Aegean islands (especially Crete) and the Greek mainland to Sicily and Sardinia (Gale and Stos-Gale 1992: 85–96) (see [Fig. 4.2](#)). Cyprus was known for its rich copper deposits so it was expected that the sampled ingots found there would have lead isotope values consistent with the use of copper drawn from Cypriot ore deposits, and it was unsurprising that oxhide ingots recovered from mainland Greece and two shipwrecks off the southern coast of Turkey shared these values. By contrast, however, the bun ingots from the Ulu Burun shipwreck were not consistent with Cypriot-source copper ore, nor were oxhide ingots from Crete. Most surprising of all, the lead isotope profile of oxhide ingots from Sardinia was consistent with Cypriot copper, and showed 'no match' with Sardinian copper sources. Instances of Greek, Cretan and Cypriot pottery found in Sardinia supported the hypothesis of a link between the island and the east Mediterranean, but this was a strange result inasmuch as the circulation of copper ingots had been assumed to serve the purpose of making copper available to regions that lacked their own ore sources.

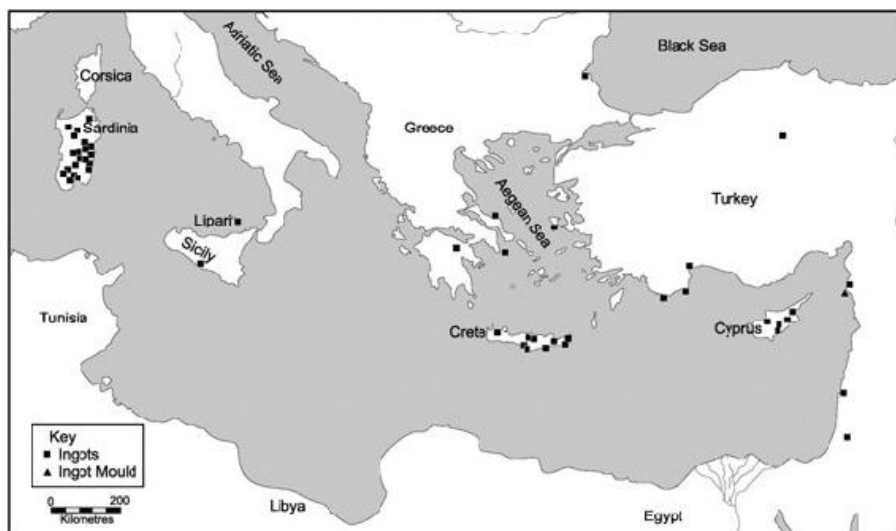


Figure 4.2 Distribution of copper oxhide ingots and ingot moulds in the central and east Mediterranean. (Source: adapted from Gale and Stos-Gale 1992: 90.)

Some archaeologists and scientists expressed caution about these initial LIA results, given the possibility that other copper sources in the Mediterranean might have lead isotope signatures that overlap those of the Cypriot ore deposits (see discussion in Gale 1991), but in the first decade of the Oxford group's research, debate was measured. Questions were raised about the number and selection of samples, and the choice of statistical techniques to define the ore fields (Sayre et al. 1992), but otherwise there was considerable agreement about the definition and overlap of these fields in the eastern Mediterranean, as well as on methodological issues. The strength of the exclusion principle was recognized, as were the advantages of LIA over trace element analyses, and despite disagreement about the extent to which an expanded range of ore analyses would clarify the definition of ore fields, there was optimism that the LIA programme would be productive.

The most pointed criticisms of the LIA programme in this period came from James Muhly (1983), who raised a number of questions about the interpretation of lead isotope profiles as indicative of provenance. He asked whether the lead isotope values determined by LIA analysis of artefacts might not be affected by the addition of lead to copper alloys, the presence of lead in the tin used to make bronze, and the resmelting and mixing of copper from different ore sources.

Gale and Stos-Gale deflected these concerns, shifting the burden of proof to Muhly: there was no evidence, they claimed, that lead other than what came from the original ore deposit had been added in the production of copper artefacts and ingots, or of resmelting before the twelfth century BCE. Moreover, there would have been no need to mix metals given the ready supply of 'rich ores' in the earlier periods of interest (Gale and Stos-Gale 1985: 90).

Within a few years the intensity of debate about LIA was heightened by a series of publications that questioned its methodology, the archaeological interpretation of its results and, ultimately, its goals. In the early to mid-1990s, a non-LIA team of archaeological scientists based at the University of Bradford published three papers that sharpened the concerns about sample selection, statistical methods and the treatment of deviant data raised by Sayre et al. (1992), and expanded on Muhly's (1983) objection that the Oxford team had not adequately dealt with the possibility of confounding factors. They argued that 'the source fields for the eastern Mediterranean are isotopically in such close proximity to one another that correct assignments of artefacts to individual ore sources would appear to be unlikely using the methods proposed' (Budd et al. 1993: 243). The implication for the Cyprus/Sardinia anomaly was that, 'in a region with so many deposits of similar geological age, the analytical resolution between different lead isotope source fields which would be needed to ascribe an origin to either Cyprus or Sardinia may not be attainable' (Budd et al. 1995a: 4–7). They criticized the Oxford group's division of 'the Cypriot field ... into five separate sub-fields representing discrete ore bodies' (p. 8), citing small sample sizes and overlaps in the lead isotope values for samples from 'mine sites' and other sources on Cyprus, and pointed out that the few published lead isotope values for samples from mines in Sardinia and Cyprus showed 'considerable overlap' (p. 12). They also noted a number of unexplained 'inconsistencies' in the Oxford group's published data on the Cypriot ore source fields and called for full publication of LIA data on all the source fields that had been analysed (p. 5). On this basis they argued that the methodology of LIA was 'fundamentally flawed' (Budd et al. 1993: 241) and did not, at that point, support a Cypriot origin for the ingots from Sardinia; north-

west Sardinia could not be excluded as a potential source for the ingot copper (1995a: 12).

Moving beyond this critique of specifics, the Bradford group also called into question the use of LIA to further the long-standing provenance agenda, urging more serious consideration of the possibility that the lead isotope profiles of artefacts might reflect ongoing practices of mixing and resmelting. As they presented it, this was not just a claim about potentially confounding factors but, rather, it presupposes an alternative model of oxhide ingot production that accords better with the results of trace element analyses as well as several lines of archaeological evidence: finds of scrap metal and indications of intensified metal production and exchange in the Late Bronze Age (Budd et al. 1995a: 22–5). Over time, the mixing of copper from more than one source would have produced an increasingly uniform isotopic composition in the ingots throughout the Mediterranean and this, they argued, would be ‘indicative of links with a highly developed, possibly economically complex trading system’ (1995a: 23). With this they emphasized the need for archaeological questions to drive the selection of materials for LIA analysis, later arguing, in more general terms, that ‘provenancing is not the only way in which to study the organization of prehistoric metal production and use’; ‘detecting change in the pattern of metal procurement and use is more useful than assigning provenance’ (Budd et al. 1996: 172; see also Pernicka 1999).

At this juncture, and as they had done earlier with Muhly, Gale and Stos-Gale (1993: 255) rejected the Bradford group’s concerns as uninformed and unsubstantiated, adding the *ad hominem* accusation that they lacked any ‘real understanding’ of the geology and mineralogy of the ore deposits they had analysed. They reasserted their claim that the oxhide ingots from Sardinia are consistent with the Cypriot ore field and not with the sampled Sardinian ores, and they invoked the archaeological finds of Cypriot pottery and metalwork on Sardinia that strengthen the case for contact (whether direct or not) between the Bronze Age inhabitants of these two islands. And they again summarily dismissed the alternative hypothesis put forward by the Bradford group on the grounds that there was ‘meagre’ evidence for recycling.²⁶

How could it happen that random amounts of copper from geographically widespread ore sources (of different lead contents and isotopic composition) were mixed together, over a long time span and possibly in quite different locations, in such proportions, so as to produce a magic mix with a narrow spread of isotope ratios which just happen to coincide with the isotopic compositions of Cypriot ore deposits? (1995: 36)

Others weighed in with analyses that supported a Cypriot origin of the copper in Sardinian oxhide ingots (Sayre et al. 1995) while Ernst Pernicka (1995) offered a balance of probabilities argument for recognizing that the mixing of coppers from different sources could not be ruled out. Midstream in this protracted debate, Christopher Chippindale, the editor of the journal *Antiquity* at the time, expressed his exasperation with an extended exchange in *Archaeometry* in these terms:

The 84 pages of discussion range across every aspect of the study, without decisive outcome. Is an ore-body actually consistent in its signature? Perhaps yes, perhaps no. How much do smelting techniques and mixing of ores blur the picture? Not sure. Do wastes and slags offer a better guide than metal artefacts? Perhaps yes, perhaps no. Which measures of ratios between the four lead isotopes should be used? For further discussion. Which statistical technique for identifying a distinct group is right? Ditto. Do the scientific results make sense in the light of other and reliable evidence? Well they *should*; that's the point of this so costly technique. (1994: 6)

Taking stock a year later, the Bradford group concluded that something fundamental was 'amiss with one of science-based archaeology's flagship projects and its promise to deliver definitive provenance for metal artefacts' (Budd et al. 1995b: 70). By 1996, the gap they had identified between the LIA results reported by the Oxford group and their archaeological interpretation of them – the claims they based on them about the production, distribution and consumption of metals during the Bronze Age in the Mediterranean – had generated a full-blown 'crisis of confidence' with respect to LIA in the UK archaeological community (Budd et al. 1996: 169). Funding for LIA research became increasingly inaccessible and by 2002 the Isotrace Laboratory at Oxford had been closed. At that point LIA research in the UK effectively ceased (although see Rohl and Needham 1998, Standish et al. 2014). As Pollard (2009) put it, 'everyone agrees that, scientifically speaking, the method works ... and yet as a

method, lead isotope analysis is hardly ever mentioned in polite company' (p. 182).²⁷

As contentious as they were, these debates did result in the iterative refinement of the analytical methodology of LIA. By establishing clear points of difference they set an agenda for research and enforced a degree of responsiveness to criticism that had often been lacking up until the second half of the 1990s.²⁸ To address concerns about inconsistencies in the LIA results published by the Oxford group, the editor of the journal *Archaeometry* agreed to compile through regular publication a standardized database of their lead isotope analyses. And in response to concerns about the sample size on the basis of which the Cypriot ore deposits were said to have been distinguished (Budd et al. 1995a), Stos-Gale et al. (1997) also published in *Archaeometry* nearly 200 new analyses of copper ores from Cyprus. Taking these results into account, and with an eye to addressing the objection that LIA could not be used to relate artefacts and specific mining sites, Stos-Gale et al. (1997) revised their characterization of the Cypriot ore deposits: rather than claiming a single Cypriot ore field, with or without subdivisions, as in earlier work, they posited several smaller fields of mines on Cyprus. Given this finer resolution of the characterization of ore deposit fields, they reaffirmed their claim that ore deposits in Sardinia have a lead isotope profile distinct both from that of the Cypriot ore deposits and the oxhide ingots found on Sardinia, and they argued that there are no overlaps between ores from Cyprus and other Mediterranean regions of the kind identified by their critics. Also, citing evidence for a mining settlement at Apliki in Cyprus that dates from c.1400 to 1150 BCE, they strengthened their earlier claim that the lead isotope results for oxhide ingots from Cyprus (c. 1250–1200 BCE) are 'consistent with' and 'match perfectly' ores from the mine at Apliki (Stos-Gale et al. 1997: 112) (see [Fig. 4.3](#)).²⁹

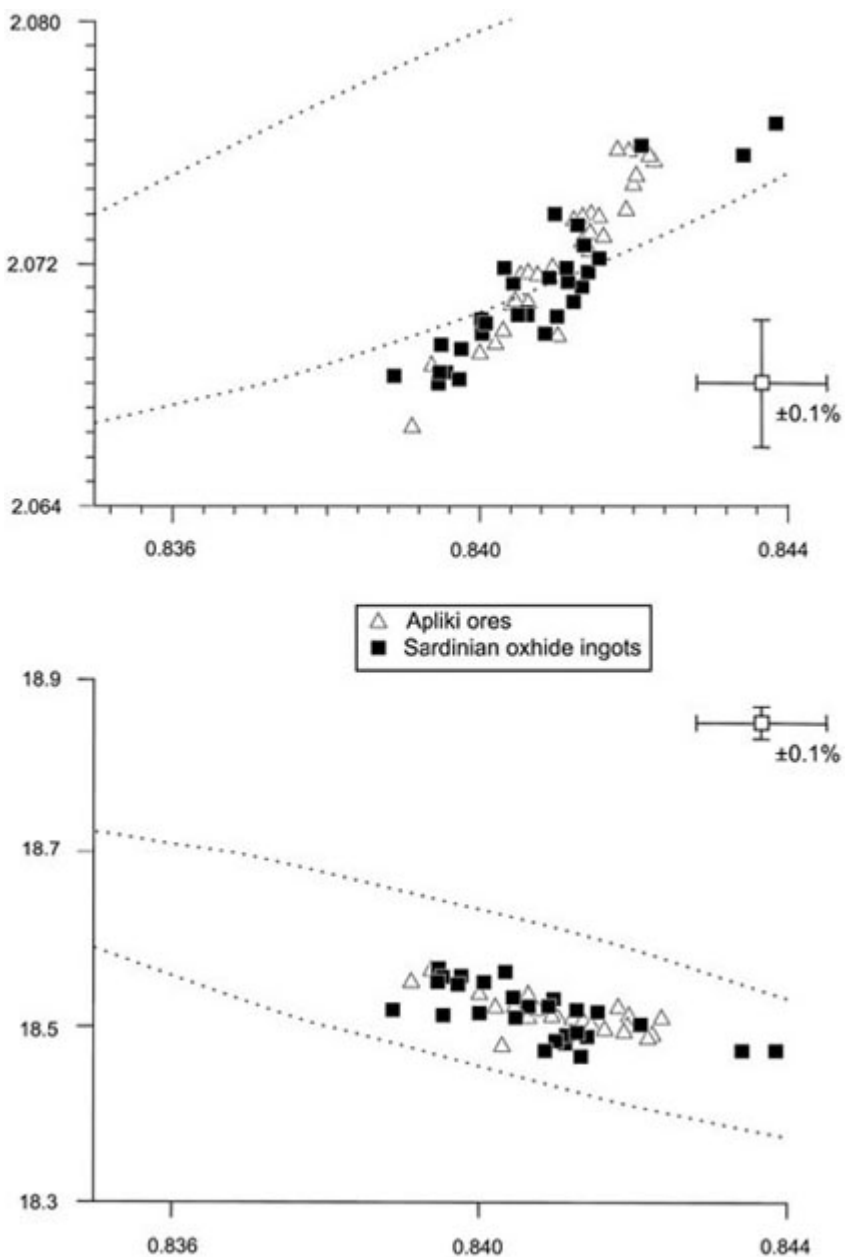


Figure 4.3 Lead isotope analysis of oxhide ingots from Sardinia and the Apliki copper ore source in Cyprus. Top: $^{208}\text{Pb}/^{206}\text{Pb}$ (vertical axis) against $^{207}\text{Pb}/^{206}\text{Pb}$ (horizontal axis). Bottom: $^{206}\text{Pb}/^{204}\text{Pb}$ (vertical axis) against $^{207}\text{Pb}/^{206}\text{Pb}$ (horizontal axis). The area enclosed by dotted lines is the original, single 'Cypriot field'. (Source: adapted from Stos-Gale et al. 1997: 116.)

For their part, the Bradford group were persuaded by this response to accept the division of the Cypriot field into 'a group of smaller fields representing individual spreading axes or even individual mines'

(Scaife et al. 1999: 124) and concluded that ‘it would seem difficult not to accept’ the hypothesis that Sardinian oxhide ingots were ‘probably’ made of copper from the Apliki mine ‘or one of the nearby mines’ in Cyprus (p. 131). With the qualification ‘probably’ they signal that they do not fully embrace the Oxford group’s most ambitious and confident claims on behalf of lead isotope provenancing;³⁰ they note that, ‘whilst all researchers acknowledge that in principle all that can be said with certainty by LIA about an artifact is that it does *not* originate from a particular ore body, there is obviously a great desire to say that it *does* originate from somewhere specific’ (p. 124; see also Gale 1999: 111–12). Finally, the Bradford group revised but did not abandon their recycling hypothesis; given evidence of an ingot being remoulded from Cypriot metal in Syria, they propose that ‘the idea of a *koine* of metal, traded but not used greatly in manufacture ... is partially correct and there is a circulating pool of metal, but it all originates on Cyprus’ (Scaife et al. 1999: 132).

Later work, as described by Pollard and Bray (2015), takes as its point of departure a decisive shift of focus away from the long-standing ‘quest for provenance’ agenda and the assumptions that underpin it. Rather than assume that the primary contribution metallurgical analysis can make to archaeology is the identification of ‘some characteristic of the source of the raw material that passes through to the finished object’ (p. 116) – a ‘chemical or isotopic “fingerprint”’ that can tie the metal content of ingots and artefacts to a unique geological source – Pollard and Bray make a case for changing the question: abandon the ‘geological determinism’ that frames provenance studies (Pollard 2011: 633, 635) and, from the outset, reconceptualize the metal artefacts under study as jointly social/technical objects. In this account the complexities of chemical profile that had been treated as confounds, factors that distort the signal of an originating ore source, become a source of evidence relevant for tracing the life history of metal objects. The chemical composition of metal objects is recognized to be unstable and dynamic, the product of ‘not merely a set of material processes but also the human decisions and structures that surround them’ (Pollard and Bray 2015: 125).³¹ Rather than focus on building a scaffolding of warrants that can control for the effects of human intervention, these become the

primary target of inquiry. This, in turn, requires the integration of multiple lines of chemical as well as archaeological evidence, a reorientation that resonates strongly with the pragmatic Bayesianism of the third radiocarbon revolution. Pollard and Bray note, for example, that repeated resmelting and recasting of copper will result in differential ‘washing out’ of the more volatile elements, like arsenic and antimony. In the case of Early Bronze Age copper assemblages, they consider a suite of elements, distinguishing sixteen different types of copper in circulation, and they construct on this basis a set of decay tables that reflect what the chemical profile characteristic of an assemblage of objects would become as the elements making up these types are differentially lost or transformed.³² These, they argue, can then be used in combination with LIA results and archaeological evidence from the excavation of mine sites, ore petrology, artefact typologies and regional chronologies to characterize the social conditions responsible for not only the production but also the use and reuse, trade and dispersal of assemblages of metal objects over time. What was at issue in the protracted UK debate about LIA, Pollard argues, was never the precision or importance of the technique but the interpretive frame that came with its single-minded application to questions of geological origin (2011: 633). The outcome of this rethinking of what the technique offers is a significant broadening of the research agenda, but one that demotes LIA results from the status of grounding a uniquely secure foundation for archaeological interpretation to one source of evidence that can be used, alongside a complex array of other evidence, to answer archaeological questions (see [Fig. 4.4](#)).

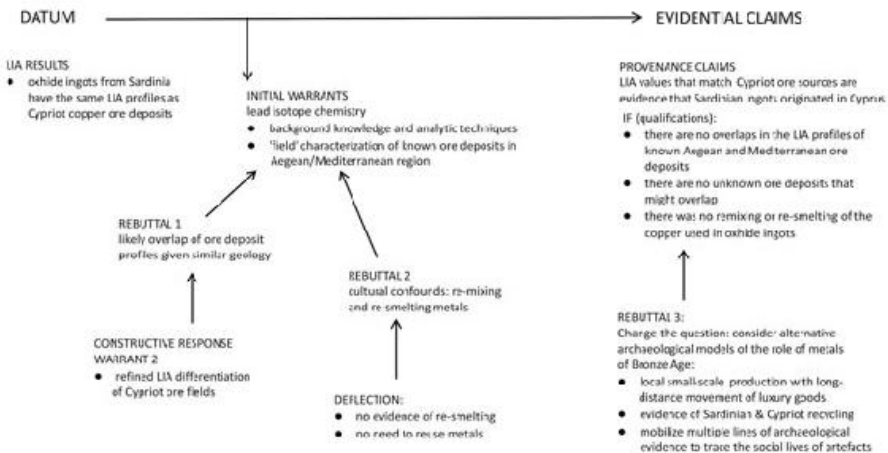


Figure 4.4 Lead isotope analysis: evidential arguments constrained by multiple rebuttals, laid out according to Toulmin's argument schema (see Fig. 1.1).

These positive developments are certainly to be applauded, but they came late and after considerable cost to the field of lead isotope analysis as a whole, at least in the UK. Reflecting on the course of the debate in the mid-1990s, Pernicka (1995) and Muhly (1995) compared the rocky reception of LIA to the introduction of radiocarbon dating: initial euphoria had given way to 'profound skepticism', and the challenge of facing up to ambiguities rather than invoking hoped-for certainties (Muhly 1995: 54). Pernicka describes this pattern in general terms: a new analytic technique is greeted with 'enthusiasm and high hopes' which gives rise to a dangerous reliance on it to the neglect of other lines of evidence, 'odd conclusions', 'over-zealous interpretation of analytical data' and 'a downward trend that can lead to frustration combined with a total condemnation of the once-hailed technique' (p. 59). This recurrent trajectory bears witness, he observes, to an underlying problem: that 'most archaeologists welcome scientific evidence but abhor scientific controversy. They want answers ... it is the answer, not the technique, that interests the archaeologist' (p. 55). In the case of LIA, this meant that analytical and methodological problems were not addressed until they became critical, long after they had been raised. Much acrimonious debate might have been avoided, Scaife et al. (1999) observe,

... if the limits of LIA would have been established early on in its history of use. The accuracy, both intra- and inter-lab, should have been established for

various grades of minerals and ores. If the use of statistical techniques really was necessary, then the nature of LIA data should have been investigated early on and techniques should have been selected that were appropriate to the data. The possible problems of anthropogenic alteration of isotope ratios should have been dealt with conclusively in the 1970s, rather than being debated in the 1990s. (p. 132)

Why was the development of a functioning trading zone around LIA so long delayed, indeed, to the point of never taking root in the UK? No doubt part of the explanation lies in conditions that fostered intense competition between UK-based research groups of all kinds for scientific archaeology funding and research reputation. But in a further critique of the Oxford group's practice of LIA, Bernard Knapp (2000) identifies, in the microcosm of this debate, an underlying problem with the interaction between scientists and archaeologists that sets in a broader context Pernicka's observations about the disconnect between what LIA could offer and what archaeologists expected of it:

Science and scientific analyses alone cannot adjudicate between cultural possibilities ... they provide analytical data which are likely to be open-ended, subject to multiple social interpretations, and in need of evaluation by collaborating archaeologists using social theory ... However precise the results of such [provenance] work appear to be, they are unlikely to make a meaningful contribution to the understanding of social processes such as production, distribution or consumption without theoretical reflection. (pp. 33, 35)

The bottom line for Knapp is that, 'no matter how complex the methods, techniques and analytical output of the physical and biological sciences may be, archaeologists alone bear the ultimate responsibility for integrating scientific results into culturally meaningful interpretations' (p. 36).

Where the specifics of the LIA debate are concerned, Knapp argues that the interpretation of a single Cypriot source for the 'production of all copper oxhide ingots that appear, analytically, to be consistent with production from Cypriot ores' is, in fact, 'inconsistent with a suite of spatial, social, historical and economic interpretations of the archaeological record': he proposes a model of small-scale, localized production at a number of smelting sites in addition to the Apliki site, many of them now destroyed or unpublished (p. 40). He also returns

to the recycling hypothesis as an alternative explanation for ‘the common isotopic signature of copper ingots found throughout the Mediterranean’ (p. 46), citing evidence for the recycling of metals on Sardinia and Cyprus (pp. 43–5). His argument for this hypothesis is detailed and immersed in the archaeological evidence; he is sharply critical of the claim for a single Cypriot source for the oxhide ingots that the Oxford group bases on their LIA results. But more fundamentally, his critique is motivated by concern that their single-source model of oxhide ingot production contradicts much else that is known about the role of metals in the Mediterranean Late Bronze Age. It assumes a mode of production that is ‘more akin to modern industries with their specialized work forces and dedicated technologies than to prehistoric realities’ (p. 40) and takes little account of the archaeological evidence that supports an alternative model of small-scale, localized production articulated in a regional economic and ideological system of long-distance movement of high-value bulk and luxury goods. In response, Gale (2001) focuses on specific points of disagreement with Knapp rather than these broader theoretical issues, but he does address Knapp’s critique of the dynamic of interaction between LIA specialists and archaeologists. He acknowledges that the results of scientific analyses cannot be assumed to take precedence over archaeological interpretation when these are at odds:

If the lead isotope data indicate something at first sight running counter to prior archaeological assumptions, then scientists must be prepared to re-examine their data and scientific interpretations of it. Equally archaeologists must be prepared to re-examine their preconceptions, which may not be so well founded on fact, as distinct from social theory perhaps not so safely applicable to ancient societies, as they had thought. (p. 122)

Nonetheless, he maintains that some issues are matters ‘to be decided purely from the scientific data’ and only when these are settled should the results be integrated into archaeological interpretation. One such matter is that of ‘ascertain[ing] the source(s) of copper for the copper oxhide ingots’ in the context of lead isotope studies:

There is no question that it is entirely unacceptable to massage the data or interpret it so as to fit in with some preconceived archaeological or social theoretic hypothesis. If it proves possible to solve the scientific provenancing

question, then it is at this stage that the answer(s) must be woven into an archaeological interpretation in social terms. (p. 125)

Knapp sees this argument for deferring archaeological interpretation until the results of the scientific analysis are established as an ‘attempt to pose a “scientific” challenge to theoretical viewpoints and social approaches in archaeology’ (2002: 37) and condemns it as ‘a predictably clichéd, often arrogant appeal for a quantitative, scientific approach; a scathing dismissal of social archaeology; and, in particular, a hyper-defensive reaction to the critique of the uses of lead isotope analysis in archaeology, or at least of the way that technique has been applied in the past’ (p. 38).

Rather than read this as a garden-variety turf war, we suggest that this exchange raises in particularly stark terms the central challenge archaeologists face in building an effective trading zone in which widely different types of expertise, and associated assumptions and methodologies, can be brought to bear on archaeological problems in ways that meet the requirements of credible robustness reasoning. Gale insists that interaction between the practitioners who play a role in generating distinct lines of evidence will compromise the integrity of their analyses and interpretations, while Knapp sees a scientist sheltering behind a belief that his quantitative, scientific methods can establish a secure, uncontestable empirical foundation for interpretation and resisting, on this basis, interdisciplinary collaboration. Knapp is, moreover, forthright in his analysis of the power dynamics that underpin this stance: ‘behind many white-coated, high-tech, mass-accelerator-driven laboratories there stands a formidable legion of power, politics and personal enhancement’ (Knapp 2002: 42). In assuming that archaeologists simply impose preconceived ideas on archaeological data, he argues, the LIA scientists fundamentally misunderstand the nature of archaeological interpretation. And in claiming foundational status for their own LIA-based evidential claims, they misunderstand their own interpretive practice. Data of all kinds, scientific as well as archaeological, stand as evidence only under interpretation and are, therefore, open to multiple interpretations. For this reason they ‘require evaluation by close collaboration and interdisciplinary discourse rather than through contestation and defamation’ (p. 38). When this type of exchange is

not successfully cultivated, Knapp observes, ‘the members of different scholarly communities frequently talk past one another, knowingly or unknowingly, because they have not been trained in the manifold and increasingly specialized techniques, approaches, viewpoints, even meta-narratives that characterize and define different disciplines’ (p. 42). For Knapp, the experience archaeologists had had with LIA specialists exemplifies this problem of communication across disciplinary ‘fault lines’. The central elements of this appraisal appear, in less combative terms, in Pollard’s retrospective appraisal of the ‘sorry history of lead isotopes’ (2011: 634) almost a decade later. He particularly draws attention to the problem that, with just three labs producing LIA results (worldwide) and a tendency on the part of some to deflect ‘helpful and constructive critique ... from outside the fraternity’, archaeological applications of LIA were compromised by a lack of robust critical debate: ‘healthy science requires debate created by a critical mass of active and informed participants, not a clique of adepts who retain sole rights of interpretation’ (p. 634).

Pulling together the threads of this analysis, what do we learn from this negative object lesson about how trade in external resources can founder and about the conditions necessary for it to thrive? First, the LIA debate in the UK reaffirms insights from the third radiocarbon revolution: the need to resist the temptation to accord foundational status to any one line of evidence, however promising the results of new technical, scientific analyses may seem. As Pollard puts this point, outside of *Star Trek* ‘instruments do not directly provide answers’ (2011: 632); they produce data that stands as evidence only under interpretation and only in relation to a particular question. As such, they cannot be presumed to ‘automatically trump other forms of evidence’; he insists that ‘all evidence must *a priori* be given equal weight’ (p. 637). Putting external technical resources to work in archaeological contexts is, moreover, a matter of mobilizing multiple lines of evidence, not just as a means of calibrating especially promising imports so they are applicable to archaeological materials and timescales, but as essential to archaeological practice, in the spirit of the pragmatic Bayesianism advocated for chronology. Establishing the archaeological relevance of technical results requires meeting the conditions for virtuous robustness reasoning described earlier. Rather

than rely on an appeal to disciplinary status and boundaries to do the work – these are, in any case, a poor proxy for epistemic credibility and epistemically relevant conceptual independence (Wylie 2000b: 311) – what is required is an explicit, rigorous, empirically grounded assessment of the reliability of each line of evidence, and of their causal and conceptual independence from one another.

These judgements of security and epistemic independence require, in turn, that the diverse expertise of each partner to the research programme be brought to bear; identifying cultural, contextual conditions that are responsible for the production and circulation of metal objects is as crucial as understanding the physical/chemical factors that affect LIA values. Contra Gale's (2001: 125) suggestion that the LIA analysis should be done first and separately, insulated from the compromising influence of archaeological interpretation, it was precisely this isolationism that prevented the questions archaeologists raised about the evidential import of LIA results from getting serious uptake in a timely way – only some of which proved resolvable by refining the isotopic analysis. This is the central lesson Pollard draws: 'there [must] be a multiplicity of voices' posing questions and interpreting the data (2011: 637). The resonance with Longino's list of procedural conditions for a well functioning scientific community is striking. The problems exposed by the debate about LIA in the UK illustrate in concrete terms the cost of failures of both transparency and responsiveness to criticism. The lack of transparency is most tangibly manifest in the criticism that the Oxford group had not systematically made all their results publicly available; the publication of their data by *Archaeometry* was a crucial condition for meeting the requirement that there be appropriate venues for criticism. It is also evident in the often remarked lack of comprehension, across disciplinary boundaries, of the standards of adequacy in evidential reasoning appropriate to inquiry in the contributing fields: laboratory-based archaeology with its roots (in this case) in physical chemistry, and anthropological archaeology. The resulting failures to give critical input serious uptake are a particular concern for fields that must function as a trading zone, especially when these failures arise from an appraisal of the social-disciplinary standing of external experts rather than a substantive assessment of

what they bring to the debate.

We add to this assessment the further point that the give and take of productive critical engagement requires communicative capacity; a necessary condition for meeting Longino's social/cognitive norms is that the parties to a debate comprehend one another's orienting goals, methodological standards, substantive assumptions and modes of evidential reasoning. In the case of a trading zone research programme, this requires, in turn, reciprocal cross-training in one another's specialisms.³³ This is what a functioning trading zone is. It depends fundamentally on the cultivation of what Collins and Evans (2007: 24–7) refer to as 'interactional expertise' and 'meta-expertise': the kind of communicative competence necessary for an outsider to understand the 'contributory expertise' of practitioners in a field other than their own. Interactional experts have 'expertise in the language of a specialism, [without] expertise in its practice' (p. 28), while 'meta-experts' operate a further remove; they have a level of understanding that puts them in a position to adjudicate expertise in fields in which they are not themselves contributory or interactional experts. The critiques of LIA suggest that it was exactly these kinds of translational expertise that were slow to develop. And the negative consequences underline the point that the successful import of external resources depends upon all parties to the exchange developing sufficient facility with the language and practice of their trading partners to appreciate what each domain has to offer, and to critically and constructively engage in the appraisal of their contributions. In particular, a reciprocity in cross-field understanding is necessary to ensure that the evidential reasoning enabled by external specialist expertise is fit for archaeological purpose: both credible in its own terms and relevant for answering questions about the cultural past. Without the resources of interactional expertise, the procedural requirements for a well functioning research community – set out as clearly by Pollard as by Longino – cannot be met and the scaffolding for what is needed to make effective use of external resources will not be built.

What the LIA case brings into sharp focus is just how many powerful forces work against the formation of a viable trading zone: disciplinary training and traditions, funding channels and institutional

structures that reinforce disciplinary divisions, not to mention the premium put on specialization in the sciences and in academia generally, as well as the entrenched hierarchies of authority among these specialisms that structure communication and exchange between them. Radiocarbon dating was the post-war poster child for peacetime applications of nuclear physics; significant and sustained investments made it possible to build the infrastructure – the technical and institutional as well as substantive scaffolding – necessary to create a vigorous trading zone that could underwrite the trajectory of its three revolutions. The difficulties encountered in establishing a comparable trading zone around LIA reinforce the insight suggested by the trajectory of the third radiocarbon revolution. A crucial condition for success in building this trading zone was the shift away from presumptions of foundationalism on all sides of the trading partnership, and the painstaking cultivation, among archaeological adopters and laboratory specialists alike, of the interactional expertise necessary to appraise what radiocarbon results can and cannot establish, with what degree of security, and to recognize what backing is required for their warrants and what rebuttals – confounds and alternative hypotheses – need to be considered.³⁴ Given that few other types of external resource have received this level of support, it is not surprising that the impulse persists to seek ‘silver bullet’ solutions to archaeological problems in a succession of external resources.

People moving: Roman Diaspora

The more circumscribed project to which we now turn – ‘A Long Way from Home: Diaspora Communities in Roman Britain’ (Eckardt et al. 2010)³⁵ – is a research programme aimed at integrating the results of stable isotope analyses that offer insight into early childhood and lifetime diet into the study of population mobility in the Roman Empire. For the Roman Empire as a whole there was broad agreement that there had been much local and regional travel as well as migration over greater distances resulting from military activity (e.g. the regular rotation in Britain of legions made up of soldiers from diverse ethnic backgrounds) and the movement of administrators and slaves, craftsmen and traders. In the early years of the Roman

conquest it was estimated that Britain saw an influx of more than 100,000 'incomers' (Fulford 2010: 68). However, a great many questions remained about the nature, causes, scale and effects of migration within the Roman Empire. The challenge taken up by the Diaspora project was, in part, to determine what isotopic analyses could contribute to answering these questions.

The basis for understanding migration patterns in later Roman Britain, the focus of attention for the Diaspora project, had primarily been evidence from epigraphy, human osteology and material culture, especially burial treatment and grave goods. Pivotal to the broader research programme were widely debated questions about whether 'foreigners' of various origins, statuses and ethnicities could be reliably distinguished from 'locals' on the basis of material expressions of their identity. In taking up the Diaspora project in the last decade, its principals, Hella Eckardt, Gundula Müldner and Mary Lewis, describe how, at this juncture, a great many simple 'post-code'-type assumptions about the identification of locus of origin and ethnic affiliation had already been called into question (2014). For example, while the evidence from epigraphs is intriguing – it indicates the origins of foreign merchants and administrators in Roman Britain and confirms that some veteran immigrant soldiers continued to live locally when they completed their army service (Eckardt et al. 2010: 102) – by no means does it provide representative documentation of a cross-section of travellers and incomers. By contrast, the skeletal material available for morphological analyses is not skewed by social status, apart from a selection bias on the part of researchers in favour of elite or exotic burials (Eckardt, Müldner and Lewis 2014: 536), and it can provide a measure of relative similarity with regional populations based on what has been called 'geographically localised aspects of human variability' (Gosden 2006: 3). However, the reference populations for making these assessments are predominantly contemporary; the population variability that can be measured overlaps on a number of dimensions; and even when physical affinity can be established between an individual and a reference population, its significance is often unclear. It cannot be assumed to indicate a direct biological relationship and it is a further step to infer cultural affiliation (Eckardt et al. 2010: 112).

For these reasons, the primary basis for identifying ‘incomers’ had been the inference of ethnicity from distinctive types of mortuary practice and burial goods. The presence of ‘exotic’ artefacts or unusual positioning and treatment of the dead, as compared with ‘local’ comparanda, was presumed to represent a ‘symbolic connection to [the individual’s] homeland’, and was treated as a proxy for ethnicity of origin (Eckardt et al. 2014: 538; see also Pearce 2010: 87). These interpretations had been widely challenged by the time of the Diaspora project. They depend on a definition of the ‘local’ tradition that risks circularity (p. 538); they ignore the possibility that returning locals might bring prized ‘exotic’ artefacts back with them or have otherwise been ‘changed’ by their travels (p. 540); and they do not consider the implications of curation by which objects that once symbolized a specific ethnicity may have lost this significance through a long history of circulation and use as heirlooms. More generally, the equation of seemingly exotic material culture and funerary treatment with foreign origins obscures the dynamism of social identity; incomers may assimilate local customs, hosts may adopt foreign traits and practices (especially those of elite incomers; pp. 538–41), and material culture may reflect other aspects of social status such as age, gender or occupation, rather than childhood origin or identification with a homeland.

The point of departure for the Diaspora project was, then, an appreciation of the contentious history of each of the lines of evidence already in play, to which they added a critical appraisal of the potential and limitations of the new isotopic lines of evidence they brought to bear. From the start this was an exercise in robustness reasoning: clearly none of the existing lines of evidence could be taken as foundational, but neither did the Diaspora project principals treat isotope analysis as a ‘silver bullet’ that could assume the status of a science-backed empirical foundation with respect to them. We focus here on their reanalysis of the late Roman site at Lankhills, Winchester, a site originally excavated by Giles Clarke between 1967 and 1972 and subsequently re-excavated by Oxford Archaeology from 2000 to 2005 (Eckardt et al. 2009).³⁶ They scrutinized the security of each line of evidence they considered, tracing the interpretive history that established the baseline for their analysis: Clarke’s claims that he

had evidence, in mortuary treatment and grave goods, of the presence of sixteen 'intrusive burials' representing incomers from the Roman province of Pannonia in central Europe,³⁷ as well as six 'Anglo-Saxon' individuals (Clarke 1977: 377, 389, as cited by Eckardt et al. 2009: 2816–17). His interpretation of the local burials had been debated since the mid-1980s, so Jane Evans, Nick Stoodley and Simon Chenery (2006) initially undertook a re-evaluation of nine 'Pannonian' and nine 'local' individuals, adding to the roster of existing evidence the results of strontium and oxygen isotope analysis. What they found was a much more complex pattern of occasional alignment and frequent divergence between these lines of evidence. In the case of four of Clarke's non-local individuals, the results of isotopic analysis confirmed that they had not originated in the region of Winchester, but they did not all have a central European isotopic signature consistent with his material culture-based interpretation; four individuals associated with 'exotic' grave goods proved to have 'local' signatures, and two of Clarke's 'locals' had non-British isotopic signatures.

Given these results, the Diaspora project expanded their analysis to include an additional forty individuals recovered in the course of the 2000–5 excavations, deliberately choosing samples of 'local' and 'non-local' individuals who varied by age, sex, mortuary treatment and grave goods. These included eight individuals who fit the criteria Clarke relied on to identify Pannonian incomers. In designing and reporting their results, the Diaspora team are careful to specify what isotopic analysis can and cannot establish. They rely on both strontium and oxygen isotope analysis, emphasizing that these are (causally) 'independent isotopic systems' (2009: 2818). Tooth enamel is tested for oxygen isotopes that reflect the composition of drinking water which is, in turn, informative of temperature and climate in the region where an individual originated, while the strontium isotope profile derived from skeletal samples is indicative of the underlying geology of the region where the food originated that an individual consumed over their lifetime. These isotopically defined regions are by no means sharply delimited given the existence of similar climates and geologies of the same type and age in different geographical regions. The scaffolding required to assess skeletal and dental isotopic

signatures includes meteorological and geological data – often in the form of geological and climatic clines of isotopic values – as well as, ideally, isotopic data from sedentary populations associated with these regions (p. 2820). So, for example, the Diaspora team could characterize the range of oxygen isotope values typical for drinking water local to the Winchester area, but they caution that this does not establish a definitive locus of origin for individuals with matching values; other areas within Britain share these values and, indeed, the oxygen isotope values for British drinking waters ‘are also found in many areas of western (Iberian Peninsula, France, the Low Countries, Northwest Germany and Denmark) and southern Europe (Italy, Greece) as well as the Mediterranean (Turkey, the Levant and even part of Northern Africa)’ (p. 2821). Likewise, although the strontium values associated with the chalk formations and sedimentary rock in the Winchester area can be ‘relatively well constrained’ to a narrow band of values, Eckardt et al. note that, given transport into the local biosphere of marine strontium (established on the basis of ‘human data’), it must be recognized that ‘these values are not overly specific’; they can also be expected for ‘other Mesozoic limestones, Palaeogene sediments and young or low radiogenic igneous terrains which are found in many areas of Britain and Europe’ (p. 2820).

Mindful of these uncertainties, the Diaspora authors caution against specifying a ‘local’ isotopic signature with which archaeological samples can be compared and, instead, focus on ‘constraining’ these values, narrowing the range of values that might characterize an individual from a given region by using the results of oxygen isotope analysis to delimit the range of strontium isotope values for tested individuals (p. 2820). Their ‘suggestions’ of local or non-local origin take the form of assessments of the relative likelihood that an individual originated in the Winchester area, or in Britain, or in various regions abroad, given ‘current best estimates’ of the range of isotope values associated with these regions. For example, individuals with tested oxygen isotope signatures that fall within the ‘local’ range are described as ‘if not from the Winchester area itself, more likely to have come from the east of the country (or compatible “cooler” regions on the European continent)’ (p. 2821).

Even given the acknowledged ambiguities of the isotope analysis,

the results of the Diaspora project analyses of individuals buried at Lankhills are quite striking (see [Table 4.1](#)). Eckardt et al. interpret them as indicating the presence of twenty-one ‘local’ individuals and eight ‘non-local’ individuals whose isotopic values are ‘consistent with a childhood in Britain or a similar climatic zone’, including the West Country and northern Britain, or Belgium or western Germany (p. 2821): incomers who ‘cluster around the edges of the UK range’ (p. 2823). They describe another eleven individuals as ‘certain incomers’ whose isotope values indicate an origin outside the estimated British range. For incomers from more distant locales, the combination of isotopic values suggests that they may have originated in the Mediterranean region (excluding the Nile valley) as well as in central Europe. Taken together, roughly half the analysed individuals were of ‘local’ origin and the other half originated in regions elsewhere in Britain and in central/southern Europe and the Mediterranean. When compared with the grave goods and burial treatment, the complications noted by Evans et al. in 2006 are amplified.³⁸ Of the twenty-one individuals who were isotopically ‘local’, just ten were associated with ‘local’ mortuary treatments while six conformed to what Clarke had identified as ‘Pannonian’ material culture and burial rites. Those from outside the British isotopic range included five individuals whose burial and grave goods would count as ‘local’ in Clarke’s criteria, and only one was associated with a ‘Pannonian’ treatment. Taking into account these inconsistencies as well as points of agreement between the results of isotopic analyses and conventional archaeological interpretation, Eckardt et al. conclude that ‘Clarke’s criteria, which have long been considered problematic, fail to predict origin’ (p. 2824). Given cross-cutting analysis of the association of isotope values and material culture with other variables, they suggest that the ‘message’ conveyed by the grave goods was not so much about an original ethnicity as about age or life-stage, marital status and kinship relations, and personal authority. Rather than seeing these discordant lines of evidence as ‘a negative result’, they argue that they reinforce the insights central to theoretical challenges that had called into question the assumption of a simple relationship between material culture (including mortuary treatment) and the articulation of ethnic identities. And this, they urge, should be treated

as ‘an opportunity to explore other explanations and social factors such as second generation immigrants, intermarriage and assimilation’ (p. 2824; see also Cool 2010).

Table 4.1 Comparison of proposed origins for individuals in the later Roman cemetery at Lankhills School, Winchester, based on archaeological and isotope analyses. The individual from grave 110 has oxygen isotope ratios outside the UK range, but these measurements may have been biased by the breastfeeding effect. (Source: data from Eckardt et al. 2009: Table 1 and pp. 2816–23.)

Origins based on isotopic analysis	
106	Cal
82	British
110	British? Breastfeeding effect
96	British?
210	Cal
226	British
265	Local British
53	Cal
55	Local British
61	British
68	Cal
79	British
80	Local British
83	British
85	British
90	Local British
93	Nonian?
97	Cal
98	Cal
99	Cal
107	Nonian?
110	Cal
116	Cal
117	Cal
118	Cal
119	Cal
120	British
121	British
133	Nonian?
136	Cal
137	Local British
138	Cal
139	Cal
140	Cal
141	Nonian?
142	Local British
143	Cal
144	Cal
145	British
146	British
147	Cal
148	Nonian?
149	Nonian?
150	Local British

The approach taken by the Diaspora project team illustrates a number of points already made. It meets the conditions for credible robustness reasoning, especially in exploiting the causal autonomy of lines of evidence based on oxygen and strontium isotope analysis, calibrating these against independent background knowledge (for example, geological and climatic evidence, isotope profiles for human populations), and treating discordant lines of evidence as raising questions that require critical appraisal of the security of each line of evidence on its own terms. This process is, moreover, an iterative one. The Diaspora team ‘respects’ (in Chang’s sense) the tentative foundations for evidential inference laid by generations of antecedent Romanists, but builds upon them in ways that fundamentally reconfigure them. This critical refashioning extends not just to specific claims of ethnic identity or locus of origin, but also to framing assumptions about the nature of ethnicity in relation to material culture, and the orienting questions of provenance, that had set the agenda for the investigation of population movement within the Roman Empire. The introduction of isotopic analysis figures, then, not as a source of uniquely secure, scientifically grounded evidence that can settle questions of origin, but as one line of evidence to be considered alongside others, and one that must be handled with care. In their treatment of the isotope results, the Diaspora team attend to a range of ‘qualifications’ (in Toulmin’s terms) – of strength and scope as well as potential rebuttals (confounds and alternative hypotheses) – which signal the operation of well functioning practices of critical engagement across the various fields of expertise relevant to assessing and applying these results in archaeological contexts.

In short, what makes possible the contributions of the Diaspora project is close, ongoing collaboration among principals who are technical specialists in several different areas, but also have substantial interactional and meta-expertise in one another’s specialisms. This illustrates the central point made, in negative terms, in connection with the LIA debate:³⁹ that in the case of a trading zone research programme which necessarily depends on mobilizing multiple lines of evidence, communicative capacity is crucial for realizing the social/cognitive norms in terms of which Longino characterizes procedural objectivity. Effective critical uptake is

impossible if partners in a trading zone lack sufficient mutual understanding to appraise claims from the perspective of their diverse specialisms. This feature of trading zone research, so clearly exemplified by archaeology, reinforces the point we made earlier: that Longino's fourth condition – the norm of 'tempered equality of intellectual authority' – must be significantly extended. It is not enough to counteract social discrimination that marginalizes critical insights and other epistemic resources within one's own research community; it is necessary, as well, to cultivate an appreciation of relevant expertise that lies with practitioners in other fields.

To put this point in more concrete terms, interactional and/or meta-expertise is necessary to meet the challenges of avoiding 'illusions of robustness' – when, for example, convergence among seemingly diverse lines of evidence is spurious – and of productively engaging discordant lines of evidence, rather than resorting to disciplinary hierarchies that privilege one as trumping the others. We argue that these challenges are best met not by isolating experts in their separate enclaves but by critical analysis, jointly undertaken, of the security, relevance and independence of the lines of evidence brought together for archaeological purposes. Moreover, this is not a luxury or an exceptional condition of trading zone contexts; it is the only way a field like archaeology can proceed. In some cases, when the joint expertise required to bring a particular external resource to bear on archaeological problems is well established, what was a trading zone will have matured to the point where the exchange becomes 'just trade' (Collins et al. 2007: 658). In other cases, what was once external expertise may have been so thoroughly incorporated into archaeological practice that there is no longer any trade involved in its use. Even so, archaeologists are constantly extending their reach to external resources, which brings into existence new trading zones, and the range of expertise they depend upon is so heterogeneous that the operation and evolution of the field as a whole depends upon cultivating effective trading zone dynamics. We take this to be generative, a source of epistemic strength rather than liability. To navigate this complex terrain successfully, however, requires a deliberately cultivated understanding of the assumptions, technical skills and conventions of practice, as well as the social and

institutional conditions that shape evidential reasoning in the diverse specialist fields and subfields that make up archaeology. This genealogical appraisal, clearly on view in the Diaspora project and in any project that makes use of legacy data, is a routine practice of ‘reflexivity made concrete’; we explore this further in our final chapter.

Notes

1

We use ‘material postulates’ here in Norton’s sense (2003), as discussed in Chapter 2.

2

Galison drew on the work of anthropological linguists to understand how trading languages take shape in physics that make purpose-specific ‘out-talk’ possible, sometimes evolving into rich, full-service hybrid languages that sustain what philosophers have described as inter-field research clusters and emergent inter-field research communities (Bechtel 1986, Darden and Maul 1977).

As later elaborated by Collins, Evans and Gorman (2007), ‘trading zones’ are defined as ‘locations in which communities with a deep problem of communication manage to communicate’; if communication is unproblematic, then ‘there is simply “trade,” not a “trading zone”’ (2007: 658). They propose a typology of trading zones differentiated by the degree to which trade is coerced rather than arising from voluntary collaboration, and the degree to which a trading zone is heterogeneous (p. 9); different strategies for sustaining trade, and different degrees of reciprocal influence between trading partners, characterize trading zones distinguished on these dimensions. This typology, they suggest, can be used to characterize the evolution of trading zones. Of particular relevance here is the potential for ongoing interaction to transform a hierarchically structured, ‘fractionated’ trading zone into a collaborative partnership in which all parties recognize the value of, and ‘attempt to gain access to’, one another’s expertise (Collins et al. 2007: 659). This may result in a situation in which there are no residual problems of communication that require the resources of ‘trading zone’ strategies for bridging the divide between contributing specialisms.

3

‘Social indicators’ is a term used by Fricker (2007) to refer to cues about the social standing and identity of an interlocutor – most obviously, their gender,

race/ethnicity, seniority – that affect the degree to which, or respects in which, we attribute credibility to them as knowers (epistemic agents). This is not a term Longino uses, but it captures the range of social determinants of credibility that she argues should not be a basis for ignoring or dismissing critical input within a research community.

4

This discussion was originally developed by Wylie for a Philosophy of Science Association symposium on ‘A New Normativity for Philosophy of Science’ (2002). The history of radiocarbon dating is an immensely complex story of enthusiasm and ambivalence, institutional manoeuvring and competition for access and authority. Marlowe (1999) gives a detailed account of its initial years; Francis (2002) outlines the impact of the first radiocarbon revolution on interdisciplinary research on Quaternary extinctions; and various aspects of this history are reported in a number of articles that assessed ^{14}C dating as it was being developed. Many of these appeared in the journal *Radiocarbon*, or in proceedings of the International Radiocarbon Conferences (e.g. Long 1992, Taylor, Long and Kra 1992, Stuiver and van der Plicht 1998) as well as in archaeological journals (Browman 1981, Bronk Ramsey 2008, Chippindale 2002, Shott 1992). Manning (2015) provides a contemporary overview of how these debates have unfolded.

5

Even so, in a review of *Radiocarbon After Four Decades* (Taylor et al. 1992), Browman (1994) observed that, while ‘error magnitude is no longer linked clearly to lab type’, differences in the standards employed by different laboratories were still an issue (p. 378). In response to these issues, Shott (1992: 219) emphasized the need for ongoing scrutiny of how different laboratories handle length of count-time, conventions for estimating counting errors, fractionation effects (a function of technique and count-time) and how they normalize results, despite the fact that, by the early 1990s, archaeologists had been advised not to worry about inter-lab variation.

6

See, for example, Gillespie’s discussion: the ‘new value is sometimes used for geophysical research but should not be used for age reports. To convert from the old to the new half-life, multiply by 1.03. There is very little point in making this correction in isolation’ (1986: 27). This is one example Francis cites of the conventions on which radiocarbon dating relies (2002: 300). See also Renfrew (1973: 288).

7

These factors change the extent to which cosmic rays are deflected before they

can reach the upper atmosphere and produce thermal neutrons.

8

For example, the 2013 issue of *Radiocarbon* is devoted to explication of the new IntCal13 calibration dataset (Reimer 2013).

9

Shott's analysis of the integrity of these ^{14}C samples is a classic example of source criticism of the kind discussed in Chapter 3; it is a matter of articulating qualifications of the scope and strength of the evidential claim in question, to use Toulmin's terminology. He is also engaged in generating and testing alternative hypotheses, a matter of addressing potential rebuttals (Toulmin 1958: 94). In the context of confirmation theory as developed by Reiss (discussed in Chapter 2), the process of establishing that these alternatives are not viable is a matter of building indirect evidence for the hypothesis Shott ends up endorsing (Reiss 2015: 347).

10

Shott cites, in this connection, what Schiffer had described as a 'strong case' approach (Schiffer 1986; Shott 1992: 203).

11

Shott refers here to estimates of typical error that, at the time, suggested that 'results even in the AD time interval can be reliably resolved only to approximately a 200-year range' (p. 226).

12

In Toulmin's terms, this point and the ones that follow – about probabilistic modelling and subjecting these models to sensitivity testing – are matters of appropriately qualifying the relevance and the scope and strength of the evidential claims based on the results of radiocarbon dating.

13

We refer here to M. A. Smith's (1955) discussion of the 'Limitations of Inference in Archaeology', discussed in Chapter 1.

14

Steel's analysis is addressed to philosophical critics of Bayesian confirmation theory, such as Mayo (1996), who argue that, in practice, scientists do not make explicit use of Bayesian methods but, rather, rely on classical statistics (Steel 2001: 153–4). The archaeological use of Bayesian statistics to estimate the relative frequency with which a given radiocarbon method can be expected to

get a calendar date right is necessary, Steel argues, to deal with the wiggles inherent in calibration curves: ‘Bayesian computational algorithms ... more easily accommodate the complexities raised by the irregular form of the calibration curve’ (2001: 160). The discussion of the use of Bayesian models and methods in the most recent *Radiocarbon* special issue on the internationally agreed 2013 calibration curves supports this claim (see Niu et al. 2013).

15

These parallels are developed in more detail in Wylie’s analysis of epistemic security and independence in evidential reasoning in archaeology (2000a, b, and 2011b).

16

For philosophical discussion of ‘security’ as distinct from robustness in the context of physical and biomedical sciences, see Stegenga (2012: 212–13) and Staley (2004).

17

Wylie characterizes this as horizontal independence between lines of evidence, as distinct from vertical independence between a test hypothesis and the evidence invoked in its support (Wylie 2011b: 381, 387).

18

Soler refers to these as worries about ‘illusions of robustness’ (2014: 210). The more specific objections she addresses, as developed by Hudson (2014), are that the evidence that is used to calibrate a measurement technique, or built into the scaffolding that enables a targeted test of contending hypotheses, should be understood to be superseded by the results of the measurement or test result that it makes possible. See Soler’s discussion of these objections as developed by Hudson (2014) in *Seeing Things: The Philosophy of Reliable Observation* (Soler 2014: 204–5).

For an especially trenchant critique of spurious convergence in an archaeological context, see Ullmann-Margalit’s analysis of a pernicious interdependence between textual and material evidence in interpretations of the Dead Sea Scrolls (2006: 40–55).

19

What ‘artefacts’ refers to in this literature are errors produced by the instruments or techniques used to record or investigate an object of study.

20

The set of conditions outlined here expands on those identified in Wylie (2000b

and 2011b), and is informed by Soler (2012: 15–22; 2014: 210–12) and by contributors to Soler et al. (2012), especially Stegenga (2012). See, in particular, their discussion of the need to ensure that each line of evidence is credible in its own right (Soler 2012: 8; Stegenga 2012: 212–13, 219), and their treatment of the conditions necessary to establish that ‘the plurality [of distinct lines of evidence] must be real and not just an illusion’ (Soler 2012: 27). In her discussion of conditions for independence between lines of evidence, Soler distinguishes between content and historical/genetic independence (pp. 27–8), two dimensions of assessment that are captured here by conditions 3 and 4. Considerations of independence between the ‘epistemic spheres’ (as Soler refers to them, 2012: 28) in which distinct lines of evidence and their warrants are developed are a particular focus of attention in Wylie’s earlier discussions of conditions for horizontal independence and are presupposed here. See also Stegenga’s analysis of independence between modes of evidence (2012: 217–19). He and Soler both emphasize the importance of recognizing that assessments of security and of independence, and judgements about how to weigh different lines of evidence, depend on context-specific considerations and come in degrees; ‘pragmatic, context-sensitive judgments are pervasively involved in scientific research, especially when degree and relevance appraisals are in play’ (Soler 2014: 212).

21

A definition of these types of expertise follows in the next section. For an extended discussion of the relationship between trading zones and interactional expertise, see Collins, Evans and Gorman (2007) and contributions to Gorman (2010).

22

Lead isotope analysis is a distinctive type of compositional analysis, initially seen as an alternative and later as an important complement to trace element analysis.

23

This research was also pursued by research groups in Heidelberg and Washington, DC.

24

See Pernicka (1999) for a later discussion of the ‘fingerprinting’ agenda.

25

This programme of fieldwork and analysis was supported by grants from the Leverhulme Trust, the UK Science and Engineering Research Council, and a British Academy funding programme aimed at supporting collaborative research

in the humanities. For an overview of the method and their results, see Gale and Stos-Gale (1992).

26

They cited the lack of evidence for the remelting of other types of ingots and scrap metal to make oxhide ingots and argued, on the basis of evidence from the site of Enkomi in Cyprus, that the copper needed for an oxhide ingot could have been produced in a single smelt. They also cited the low frequencies of tin ($< 0.05\%$) in oxhide ingots compared with expected higher frequencies (c. 5%) if bronze artefacts had been recycled (1995: 33–5).

Tite (1996: 959) also criticized Budd et al. on the grounds that their papers ‘contain very little, if any, new scientific data but instead attack established procedures and interpretations’ (p. 959); there was no experimental research supporting the recycling hypothesis, and Budd et al. ‘often seem to overlook the original authors’ qualifications on their results and interpretations’ (p. 961).

27

In contrast, LIA is widely accepted and collaboratively practised in continental Europe in areas from Scandinavia in the north, through Germany and France to Spain in the south. Its capacity to disrupt archaeological understanding is shown by examples of object production that were not based on local copper ores or silver sources, for example, in the Scandinavian Bronze Age (Ling et al. 2014), the Argaric Bronze Age in south-east Spain (Lull et al. 2014) and the majority of the silver vessels from the Shaft Graves at Mycenae (Stos-Gale 2014).

28

Pollard (2011) observes, in an appraisal of lessons learned from the LIA debate, that one factor responsible for its counterproductive trajectory was the fact that very few laboratories were doing lead isotope analysis and ‘some were less good than others at publishing their raw data’ (p. 634).

29

Stos-Gale et al. (1997) also claim that the values for Apliki ore ‘plot together with’ that of ingot fragments from north-west Sardinia, and ‘match closely’ the values for ingots from mainland Greece, Crete and even Bulgaria. Gale (1999) adds further analyses that suggest that different copper ore sources might have been used at different times to produce oxhide ingots in Cyprus, Crete and those found in shipwrecks off southern Turkey.

30

Stos-Gale et al. (1997) conclude that they are confident that ‘with sufficient well-controlled data acquisition, the field of lead-isotope provenancing is one of

the more successful fields of science-based archaeology' and the 'crisis of confidence' in LIA is not justified (p. 119).

31

Pollard objects that, in fact, refining the precision with which the provenance of a metal object can be traced to an originating ore source may not contribute all that much to archaeological understanding: 'how valuable is it to know where something/someone comes from?' (2011: 635). Perhaps the source of the ore mattered socially and culturally, but this is not necessarily the case. Questions of relevance arise here that parallel those which have long been a focus of attention in refining archaeological uses of radiocarbon dates: how does isotopically identifiable provenance relate to the culturally significant practices of production, exchange and use of metals in prehistoric contexts?

32

Another target of Pollard and Bray's (2015) critique of the provenance agenda is its preoccupation with tracing specific artefacts to the ore deposits in which their metal originated. They urge a shift of scale from analyses aimed at identifying the chemical signature of individual artefacts to characterizing the chemical profile of assemblages of objects.

33

Pollard's goal, in his 'cautionary tale' retrospective, was to open up 'dialogue between laboratory and field archaeologists' about what can realistically be learned from isotopic analysis and about the 'complexity of archaeological interpretation' (2011: 631). He stresses the point that 'science at its best is collaborative' (p. 634); we build on this point by identifying some of the conditions necessary for effective critical, collaborative engagement across subfield and disciplinary boundaries.

34

The successive refinements of calibration curves illustrate the breadth of additional external expertise that had to be recruited in the course of the second radiocarbon revolution to make archaeological applications viable. Shott's Childers Site analysis, undertaken at the point of transition from second to third radiocarbon revolution, illustrates the depth of interactional expertise required of archaeologists operating in a pragmatic Bayesian spirit to make effective use of this array of contributing specialisms.

35

The Diaspora project was funded by the Arts and Humanities Research Council of the UK from 2007 to 2009.

36

Several other related analyses are reported in Eckardt et al. (2010), Chenery et al. (2011), Chenery et al. (2010), Leach et al. (2010), Leach, Lewis, Chenery et al. (2009) and Müldner et al. (2011).

37

Eckardt et al. describe Pannonia as ‘correspond[ing] roughly to areas in modern day eastern Austria, western Hungary and the northern Balkans’ (2009: 2817).

38

According to archaeological criteria, such as the presence or absence of particular grave goods and of coffins, half the individuals sampled would have been judged to be local, eight from Pannonia, and twelve identified as ‘other’.

39

We note that Pollard’s aim in drawing lessons from the ‘cautionary tale’ of lead isotope analysis was to foster just this kind of discerning appraisal of the potential and limitations of ‘the latest fashionable isotope system (strontium in dental enamel)’, so that ‘this very promising technique [might avoid] suffering the same fate’ as LIA (2011: 631). We see the Diaspora project as exemplifying the type of cross-field communication and collaborative practice he advocates.

Conclusions: Reflexivity Made Concrete

The starting point

We began this book by stressing that material traces can provide evidence that has the capacity to challenge existing understandings of the cultural past, whether these are Trojan treasure or human remains that bear witness to a human antiquity that was far deeper than could be imagined within the frame of biblical ‘begetting’. The process of turning these material data into evidence is not simply a matter of physical discovery: the data do not speak, our discoveries are mediated by interpretation, and our observations and interpretations require various kinds of scaffolding in the form of theory, background knowledge, technical skill and social and institutional infrastructures. All these resources are themselves subject to continuous revision and refinement, giving rise to competing interpretations as often as they help to resolve outstanding questions. What archaeologists rely on, in the painstaking process of assessing and delimiting interpretive options, are strategies of working with multiple methods and lines of evidence. The result has been a number of striking successes: in some cases decisive reversals of entrenched wisdom; in others, an appreciation of the complexity and diversity of the cultural past that sometimes opens up whole new domains of inquiry. We asked how these ‘successes’ are realized; how does this shaky, diffuse evidence on which archaeologists necessarily rely ever justify confidence in their representations of the past?

Our approach to this question has been to attend to the wisdom embodied in practice; our aim has been to learn from the collective experience of creative and skilled archaeologists rather than retreat to idealized accounts of how ‘science’ works. In this we draw inspiration from David Clarke’s (1973) cautionary warnings about the imposition on archaeology of models derived from physics and his advocacy of an internal philosophy of archaeology, and from Stephen Toulmin’s (1958) arguments for attending to ‘logic in use’. Philosophy can be a useful guide in navigating a path through epistemic debates, as we

argue in [Chapter 1](#), but, taking our cue from the ‘practice turn’ in the philosophy of science, the analysis we develop in this book is anchored in close-to-the-ground case studies: of fieldwork in [Chapter 2](#); of strategies for working with old evidence in [Chapter 3](#); and of the trading relations that bring external resources to bear on archaeological problems in [Chapter 4](#). By no means do these cases exhaust what archaeologists do with evidence; there are many other forms of fieldwork than excavation, and many dimensions of material analysis we have not considered. But this is, we hope, a rich beginning that illustrates what is to be gained by shifting focus away from epistemic ideals of truth and objectivity and considering a range of no less philosophical but more pragmatic epistemic virtues that are manifest in archaeological practice.

By way of conclusion we expand discussion on four topics: the paradox of interpretation, a pragmatic alternative to this paradox, the reconceptualization of objectivity, and the role of situated knowledge in concretely grounding reflexivity.

The paradox of interpretation

As we argued in [Chapter 1](#), there is considerable unfinished business in archaeology’s engagement with evidential reasoning. The recurrent crisis debates of the twentieth century culminated in a starkly oppositional ‘science wars’-style conflict that pitted constructivist critics against the advocates of a positivist New Archaeology in the 1980s. This conflict has subsided, but not because the underlying epistemic issues have been resolved.¹ Archaeological debate continues to be structured, on the one hand by the pull of ‘scientific’ ambitions construed in terms of deductivist and empiricist ideals of certainty and, on the other hand, by epistemic anxieties about the insecurity of virtually all archaeological knowledge, even the most seemingly stable empirical evidence. But if there is no ‘aperspectival’, value and theory-free, self-warranting or otherwise foundational body of empirical ‘facts’ to ground evidential claims, and no hope that the material traces archaeologists do capture will sustain these claims with deductive certainty, then what is to be done? Contrary to the assumptions that generate the recurrent dilemmas of crisis debates

and that haunt many close-to-the-ground methodological disputes, we argue that a realistic appraisal of the complexity and uncertainties of evidential reasoning in archaeology is not a brief for epistemic pessimism; certainty may be out of reach but unconstrained speculation is by no means the only alternative left standing.

If evidential claims are understood to be inferential constructs – the conclusions of inductive arguments (often implicit) that depend on a scaffolding of substantive warrants – they are all fallible, as would-be scientists such as M. A. Smith (1955) and post-processual constructivists alike have argued. However, the ‘element of conjecture’ that so concerned Smith is not, as she asserted, strictly ‘untestable’. As we argued in general terms in [Chapter 1](#), the warrants for these inferences and their backing can be held accountable empirically, conceptually and pragmatically. Moreover, although interpretation reaches down to the level of observation, a point we illustrate through analysis of excavation practice in [Chapter 2](#), it is often possible to read a heavily constructed empirical record against the grain. As we argued in connection with uses of legacy data in [Chapter 3](#), evidential claims can be systematically appraised and their credibility reinforced or undermined in ways that generate new insights, sometimes upending the very framework assumptions that informed their capture in the first place. This can be seen in practical terms in the example of Clarke’s provisional models of Glastonbury, every element of which has been productively subjected to critique, and it is clearly evident in the case of the iterative reassessments of the ‘Eminent Mounds’ of the central waterways of the US that have called into question simplistic claims about their status as mortuary sites and the terms of debate about their place in a trajectory of cultural evolution. The ‘ladenening’ of archaeological claims with ‘theory’ (to use Hanson’s terminology, 1958: 19) need not be viciously circular, a matter of projecting just what you want to see onto an obligingly accommodating screen of enigmatic, empirical data. Therein lies the paradox of interpretation; enigmatic though they are, the surviving material traces of past actions and events, lives and contexts have a capacity to resist ‘theoretical appropriation’ (Shanks and Tilley 1989: 44), and it is this that, at its best, archaeological inquiry successfully exploits.

A pragmatic alternative

Rather than wrestle with the dilemmas generated by ideals of deductive certainty and ‘aperspectival’ objectivity, we have argued that a pragmatic turn is called for.² We recommend setting realistic epistemic standards that can function as a regulative ideal for the ongoing, messy practice of building, adjudicating and using archaeological evidence – standards that are, themselves, understood to be evolving, subject to critique and refinement. The fragmented, radically diverse nature of the material traces with which archaeologists work, often lamented as imposing insuperable limitations on archaeological practice, is a crucial resource for strategies of evidential reasoning that are almost invariably more like cables than chains. The robustness reasoning we described in [Chapter 4](#) depends on the causal and consequent conceptual and technical heterogeneity of the diverse lines of evidence that can be built up from this mass of disunified material traces. These strategies of mobilizing multiple lines of evidence are vividly on display not only in pragmatic Bayesian approaches to radiocarbon dating and the Diaspora project investigation of population movement in Roman Britain, but also in the uses of legacy data we considered in [Chapter 3](#).

What lends evidential claims credibility in the context of the opportunistic, multifaceted research programmes required to mobilize these traces as evidence is not grounding in a bedrock of incontrovertible observations, as required by what John Norton describes as a ‘hierarchical form of empiricism’ (2013: 18),³ but the density of a ‘highly connected, massively tangled structure’ of inductive support for these claims that comes from many different directions (p. 3).⁴ In such a structure, theoretical and empirical elements support one another in the way that the constituent building blocks of a masonry dome, or in more limited terms, a stone arch, are self-stabilizing (pp. 19–21), but with the added resource of mutual constraint and support drawn from a ramifying network of exchanges between research fields. The key consideration in assessing such a tangle of support, brought into sharp focus by the philosophical debate about robustness reasoning, is that it should not be spurious; the convergence of elements that support one another should not be

an artefact of shared assumptions or realized by means of finagling that arbitrarily trims and fits them to one another. To assess these tangles, then, requires that archaeologists bring to bear as many different types of expertise as are needed to appraise the security of each line of evidence on its own terms, as well as to assess the causal independence of the processes that generated the anchoring traces and the conceptual independence of the background knowledge and analytic methods that warrant their interpretation as evidence. What we need, then, are norms of practice that provide guidance for appraising the integrity of the trading zone practices required to build and to adjudicate the robustness of these ramifying tangles of evidence.

Objectivity reconceptualized

Our proposal is that ideals of ‘objectivity’ be reconceptualized in procedural terms, as outlined in [Chapter 4](#). To clarify what this involves it is important to disentangle the various senses in which we use the term ‘objectivity’. It is not only a palimpsest of accumulated historical meanings, as Lorraine Daston and Peter Galison have argued (2007), but it is also systematically ambiguous conceptually (no doubt as a result of this history). As a concept that denotes epistemic success we attribute ‘objectivity’ to, or withhold it from, such diverse referents as objects of knowledge, knowledge claims themselves, and epistemic agents (knowers), both individual and collective. So, for example, we characterize objectivity in these terms:

O¹: objects: the ‘really real’, objects of knowledge that exist and that have the properties they do, independent of us and our knowledge of them;

O²: individual epistemic agency: impartial, unbiased knowers to whom something approximating a ‘view from nowhere’ can be attributed;

O³: collective epistemic agency: procedural integrity in the form of community practices that counteract not only the idiosyncratic bias of its members but also various forms of insularity and distortion that can arise at the level of research communities themselves from their social dynamics and processes of collective deliberation;

O⁴: epistemic virtues, the good-making qualities of knowledge claims: standard

lists of these include, for example, empirical adequacy (breadth and depth), internal coherence, external consistency, explanatory power and sometimes aesthetic and heuristic virtues such as simplicity or computational tractability.⁵

Disputes about objectivity in the first sense (O¹) dominate theoretical debates about the ontological nature of the cultural subjects that archaeologists study, including everything from seemingly functional tools to human agency, settlements and social enclaves to cultural systems. We set these aside for present purposes and concentrate on conceptions of objectivity that apply to knowledge claims about these subjects and to the epistemic agents, individual and collective, engaged in investigating them. The pragmatic dimension of our account is captured by an appreciation that the virtues which make up the epistemic sense of objectivity (O⁴) are themselves open to interpretation and are evolving. For example, what counts as empirical adequacy is in part a function of available technical and empirical resources, and also of the goals of inquiry: which problems need to be resolved, with what degree of precision or reliability. Also, as Thomas Kuhn has famously argued, these diverse epistemic virtues typically are not jointly realizable (1977: 324–5). Priorities must be set and trade-offs made between, for example, the reach of an explanatory principle and its empirical adequacy to particulars in the domains to which it applies, or, between the virtues of consistency with established bodies of knowledge and those of empirical adequacy or explanatory power, as when strikingly new hypotheses are introduced that challenge conventional wisdom. The proposal, based on lead isotope analysis, that Sardinian oxhide ingots were made of Cypriot copper ore (discussed in [Chapter 4](#)) is an example of this last that illustrates a further complication, ubiquitous in archaeological contexts: the question of whose established wisdom should be treated as authoritative and whose is open to reassessment in light of a renegade hypothesis supported by anomalous evidence.

In short, there is no context, subject or problem-independent set of considerations that determines where the bar should be set with respect to the epistemic virtues that constitute objectivity in the fourth sense. To conclude that a particular knowledge claim meets O⁴ norms of objectivity is to determine that they are as robust and reliable as we can make them, given available resources and with reference to

intended uses; they are trustworthy as a basis for further inquiry or for action of various kinds.⁶ It is important, then, to disaggregate the cluster of epistemic virtues associated with objectivity in the fourth sense and ask, with respect to particular contexts of inquiry and use, the following questions. What makes for reliable knowledge that is fit for purpose? What do we need knowledge for? And what types of error are most important to avoid? As technical, empirical and conceptual scaffolding is built and refined, it becomes possible to set new thresholds of adequacy for answers to old problems and to address new problems that had been out of reach, rebalancing and reinterpreting these constituent virtues of O⁴ objectivity in the process. This bootstrapping process is evident in the successive iterations by which increasingly accurate, well substantiated depositional patterns were teased out of the seemingly ‘amorphous agglomeration’ of data recovered by nineteenth-century excavations at Glastonbury.⁷

Situated knowledge and reflexivity

Often an ‘aperspectival’ ideal of objectivity, construed as a requirement that individual epistemic agents embody a stance of unbiased detachment (O²), is taken to be a condition for meeting or, indeed, a proxy for, ideals of objectivity in the epistemic sense (O⁴). The knowledge claims produced or endorsed by seemingly impartial epistemic agents are assumed to be trustworthy, in the sense of both honestly conveyed and epistemically reliable, while it is assumed that the interests and distinctive perspectives of knowers who are seen as partial cannot but compromise the credibility of any claims they put forward. The issue here is, what counts as partiality? Often perceptions of partiality are based on social indicators of status; they reflect appraisals of a knower’s fit with dominant cultural norms. Everyone draws on distinctive bodies of experience, background knowledge, interpretive heuristics and skills as an epistemic agent; in this sense, all knowers are ‘situated’. What varies is the degree to which the contingent histories and interests that constitute an array of epistemic stances are taken for granted.⁸ For some these will be all but invisible, at least to insiders, because they conform to expectations

about who is authoritative or trustworthy, whether these are specific to the culture of a particular discipline or the wider society, while others are, to varying degrees, highly visible, marked as divergent from these dominant norms. Critiques of the assumption that objectivity requires an escape from the limitations of any specific social or epistemic location – the aperspectival ideal we introduced in [Chapter 2](#) – are well rehearsed in archaeological contexts, as elsewhere. There is no ‘view from nowhere’ to which we can meaningfully aspire (Nagel 1986); the assumption that some have achieved this feat of transcendence typically obscures the specificity of the situated interests and angle of vision they bring to their epistemic projects.

As with constructivist critiques that bring into focus the situated nature of the research traditions we have examined, we argue that this more general ‘situated knowledge’ thesis does not entail a corrosive, all-consuming relativism. Contrary to the assumption that our inability to bracket all social values and transcend context-specific norms and interests opens the door to dogmatism or to the free play of speculation, the cases we have analysed bring into focus a number of ways in which the interplay of the insights, expertise and angles of vision brought to bear by differently situated investigators can be a powerful corrective to localized error and systematic bias (e.g. the different interpretations by Collingwood and Bersu of the excavated features at King Arthur’s Round Table in [Chapter 2](#)).

The capacity to mobilize the resources of many different kinds of situated knowledge is especially important for a field such as archaeology, which must adjudicate a great many different kinds of evidential claims and warrants to build robust tangles of evidence. Here we extend the point we made in [Chapter 4](#) about the challenges and the virtues of trading zone practices that integrate diverse types of external expertise. We include among these resources the experiential knowledge that arises from distinctive social identities and locations as well as the field-specific technical expertise and theoretical perspectives we discussed in [Chapter 4](#). The principle at work here, developed most systematically by standpoint theorists, is that knowers who are most readily seen as falling short of ideals of impartiality and detachment are often especially well positioned to identify and

question taken-for-granted assumptions that are no longer visible to insiders, whether these are embedded in norms of data-gathering practice and typological conventions, or embodied in orienting questions and framework assumptions (Wylie 2012: 61–2). In this account, interactional expertise is required not only to ensure that the exchanges with specialists in external fields such as chemistry, climate science and ecology are well informed and reciprocal, but also to enlist the insights of critical insiders and non-scientific outsiders who may have the advantage of critical distance when it comes to identifying and scrutinizing assumptions that disciplinary insiders take for granted. The critical mass of women entering archaeology in many contexts in the 1970s and 1980s brought to bear a grass-roots awareness of the contingency of gender roles, identities and institutions that was pivotal in productively disrupting gender-normative assumptions. Once named, these could be seen to have informed everything from attributions of function to sites and artefacts to influential accounts of major cultural transitions and field-defining research agendas (Wylie 1997, Conkey and Wylie 2007). Likewise, the diverse initiatives that make up the growing field of community-based collaborative practice in archaeology are advocated not only because they are ethically or politically appropriate, and sometimes legally required, but also because they enrich archaeology epistemically. Some of the most compelling examples are collaborations with Indigenous descent communities whose distinctive bodies of experience and traditional knowledge make it possible to develop new lines of evidence and who bring a fresh perspective to bear on entrenched norms of justification and research priorities (Atalay et al. 2014; Silliman 2008a and 2008b; Wylie 2015; Nicholas and Markey 2015).

It is the distinctive standpoint of these external partners and inside-outsiders that puts them in a position to take critical distance from elements of scaffolding – conventions of practice and theoretical assumptions, disciplinary cultures and institutions – that have come to be accepted as just what it is to do archaeology and, in this, become insulated from critical appraisal. This critical distance can be realized in other ways, for example by cultivating the wisdom of hindsight in the form of critical histories – genealogies – that trace the origins and

trajectories of now-dominant modes of practice. A case in point, which we discuss in [Chapter 2](#), are the critical histories that bring into focus the tenacious legacy of nineteenth-century social evolutionism that still configures modern excavation strategies and reporting conventions, even though the defining problems and theoretical assumptions that gave rise to these practices have long since been abandoned. Empirical studies (social histories, sociologies, ethnographies) of the disciplinary cultures, economic conditions and institutional and social/political settings that have shaped archaeologies of various kinds bear witness to the contingent factors that send archaeological inquiry down one path rather than another, entrenching elements of scaffolding that both enable and delimit the range of questions asked, the types of data gathered, the lines of evidential reasoning pursued and the range of external resources recruited.

Such analyses are stock-in-trade for many archaeologists who press legacy data into service; it is essential to understand the ambitions, the vision, the resources and the practices of those who originally recovered these data if they are to be effectively mined for new evidential insight. Critical histories and institutional analyses also routinely figure in the formation of new research programmes. Critics of the class-based biases of mainstream archaeology investigate the jointly social and political histories of archaeology (e.g. McGuire and Paynter 1991; Patterson 1995; Trigger 1996); the advocates of an explicitly feminist research programme in archaeology recover a lost history of women's contributions to the field, and analyse the mechanisms that continue to deflect them from archaeology (e.g. Claassen 1994; Claassen and Joyce 1997; Moser 2007); social histories expose the legacy of nationalism, colonialism, settler regimes and race politics in the agenda and practices of contemporary archaeology (Chadha 2002; Kohl and Fawcett 1995; Meskell 1998; Orser 2004). These are not valorizing internal histories of the kind that were pilloried in the early 1970s by Robert Schuyler (1971). They are probing, substantive appraisals of conceptual, technical and institutional scaffolding that, in the view of the authors, has become entrenched in counterproductive ways; as such, they play a crucial role in exposing pernicious tangles and opening up new investigative

horizons. It is this practice of empirically grounded meta-analysis that we refer to as ‘reflexivity made concrete’: a matter of continuously testing and refining the tools in use, keeping their problem-specific contingency firmly in view. Such practices, we argue, are indispensable to the bootstrapping processes of epistemic iteration we find especially productive in archaeological contexts – and in this they are essential for realizing objectivity in a pragmatic, procedural sense.

What matters, then, is to cultivate norms of transparency and critical engagement that make for a well functioning research community, one that can be trusted to produce knowledge of the past on which we can rely. At the centre of a procedural account of objectivity in the third sense (O³) are norms of practice designed to ensure responsiveness to criticism and transparency with respect to the empirical and conceptual resources from which tangles of evidence are built. We identified several such social/cognitive norms in [Chapter 4](#), extending Helen Longino’s account to encompass conditions necessary for fostering effective critical engagement in the context of ‘trading zones’. Chief among these is the cultivation of the cross-field communicative skills (interactional and meta-expertise); the reciprocal uptake of criticism from diverse sources is only possible given understanding of the distinctive aims, assumptions and standards of practice associated with these contributing specialisms. These norms for a well functioning epistemic community also reframe the question of which epistemic virtues should be cultivated at the level of individuals: what counts as objectivity in the second sense (O²). A number of these have surfaced in the case analyses, most prominently virtues of epistemic humility and a commitment to ‘honor ambiguity’ (Gero 2007). At both the individual and the collective level, our emphasis has been on epistemic virtues that will ensure the dynamism as well as the integrity of the research enterprise: honouring the scaffolding that makes possible the pursuit of a rigorous, focused research programme, but keeping in view the contingency and purpose specificity of the framework assumptions and norms of practice that function as its tentative foundations. Reflexivity in the concrete sense we advocate is itself an empirically grounded research practice, and one that is essential for securing the possibility of transformative critique.

Knowledge worth having

There are a great many other aspects of archaeological practice to consider than are represented by the selection of cases around which we have built our analyses of excavation practice, strategies for making effective use of legacy data, and the trading zone dynamics that make it possible to recruit external resources. By sampling the wisdom in practice represented in these areas we hope to have established that general claims about the epistemic standing of archaeology – sweeping endorsements or indictments – are unsustainable. There is archaeological knowledge worth having that falls short of certainty, but the task of characterizing what makes for more and less credible knowledge requires discerning analysis of the epistemic wisdom embodied in practice. This is a dimension of archaeological practice that is already thriving in many contexts; embedding firmly in the disciplinary scaffolding of archaeological practice is, we argue, the way forward for productive work on, and use of, evidential reasoning in archaeology.

Notes

1

We cited Johnson's (2010) argument when we discussed this debate in Chapter 1. He describes his frustration with a widespread pattern of evasion of these issues in the early 2000s: 'angry claims and counter-claims about relativism and its dangers had largely subsided', but what took the place of 'intelligent and informed evaluation of the arguments' were unargued assertions of 'a middle ground' or of a live-and-let live pluralism: 'there is something to be said for both sides' (p. 223). Although he sees a promising shift in focus in theoretical thinking about the nature of the cultural subject (e.g. theories of cultural process and agency) – he cites Hegmon (2003) in this connection (pp. 223–4) – his analysis of the epistemic dimension of theoretical debate in archaeology is consistent with our assessment. He argues that, rather than construe this 'theoretical struggle' as a conflict between processual and post-processual 'isms', it is better understood as a conflict between 'a gut attachment to a naïve empiricism, and a cerebral insistence that we are all theorists' (p. 235). Our aim has been to understand what is at stake in this struggle and to articulate a systematic account of epistemic options that are obscured by theoretical debate

when it is structured by these dichotomous intuitions and associated ‘isms’.

2

For an analysis of what a ‘[pragmatist theory of evidence](#)’ requires, and the contrast with an experimentalist paradigm in terms of which it is defined in philosophical contexts, see Reiss (2015).

3

Norton describes this empiricist foundationalism in similar terms to Johnson (2010) in an archaeological context: ‘This hierarchy starts with observational propositions; above them come propositions inferred from the observational by induction; then further propositions inferred from them; and so on. The hierarchy is structured by distance from observation’ (2013: 17).

4

Nancy Cartwright makes this point in especially compelling terms with respect to the question of what scientists know about causality and in critique of the standard hierarchies of evidence endorsed by the advocates of evidence-based policy (2013). In developing a critique of overconfidence in the evidence generated by policy research modelled on randomized clinical trials (RCTs), she stresses the importance of distinguishing between evidential ‘clinchers’ and ‘vouchers’ (p. 10); RCTs may ‘clinch’ the claim that an intervention is effective in the test population but not do more than vouch for its generalizability to other contexts.

5

Overlapping lists of epistemic virtues captured by the honorific ‘objectivity’ have been proposed by philosophers and historians of science as diverse as Kuhn (1977: 321–5), Longino (1990: 77) and Dupré (1993: 11).

6

For further discussion of the view that the epistemic virtues of ‘objectivity’ can be usefully understood in terms of epistemic trust, see Scheman (2001) and Grasswick (2014).

7

In this case, discussed in Chapter 3, Clarke’s admittedly abstract and tentative ‘Brownian motion’ model of the post-depositional processes affecting the Glastonbury site served as a productive framework for his initial analysis, and became the point of departure for trenchant critique and reanalysis that led to its replacement by more sophisticated, locally accurate models.

For a more detailed account of this ‘situated knowledge’ thesis and the standpoint theory we draw on to articulate implications relevant to this discussion of ‘reflexivity made concrete’, see Wylie (2012).

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